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NOTES AND COMMENTS.

Franz Josef Land and its Explorers.

In reply to our note entitled "The Little Trip of the Fram," Mr. Montefiore Brice has favoured us with a lengthy and characteristic epistle, to the details of which we reply elsewhere. We are, however, sure that Mr. Brice would wish us to give equal prominence with our original note to a disclaimer of any wish to attack his friend Mr. Jackson, either for the work he is doing, or for sentences in his private correspondence. The person we felt called on to criticise was Mr. A. Montefiore Brice. According to Mr. Jackson's letter, the true position, by latitude and longitude, of Nansen's winter hut was as far from Nansen's idea of its position as Paris is from Prague. What was Mr. Brice's object in publishing that paragraph? Many people thought he meant to insinuate that Nansen's latitudes and longitudes could not be relied on. And we objected to discredit being cast on Nansen by quotations from the private letters of "a man who is," as Mr. Brice insists, "still at work in the Arctic Regions, and unable to reply for himself" to the questions that are being asked.

No amount of effusive laudation in other parts of Mr. Brice's paper could prevent this damaging inference; and while we are only too pleased to learn from him that this inevitable conclusion was incorrect, we cannot exonerate the Council of the Royal Geographical Society from publishing these incompletely proved statements, which might so easily have been laid hold of by would-be slanderers of Nansen. Preliminary notices of this kind are as objectionable in Geography as they are in Zoology.

Mr. Brice quotes the president of the Geographical Society as saying that "the value of such a survey" as Mr. Jackson's "can hardly be exaggerated." But Sir Clements, it will be noted, used the future tense; and when at some future time we have Mr. Jackson's results properly put before us, we shall be better able to appraise their precise value. For the present, we need only compare them with the magnificent opportunity offered him for obtaining information of great

geographical value. What geographers wanted to know about Franz Josef Land, was its extension to the north and to the east, and whether Wilczek Land was a small island or the western edge of a great land-area. This was the main problem that we hoped to see solved by the expedition so generously equipped and supported by Mr. Harmsworth. But the moment Nansen's map was flashed on to the screen at the Albert Hall, it was clear that it was he who had solved the problem, showing Wilczek Land to be a small island, and the Dove Glacier non-existent. This he has done as a parergon, in the course of doing what most people consider a good deal more than "only extending our knowledge a few degrees in one direction." To place the revision of local topographical details and of their nomenclature by the side of this, surely is (*pace* Sir Clements Markham) gross exaggeration.

As for Mr. Brice's facile sneers at anonymous attacks, we may point out that anonymity does not carry with it irresponsibility; the Editors of this Review accept the fullest responsibility for these editorial Notes, and communications will always find them at the address given on the wrapper and last page of each number.

THE LEARNED SOCIETY BORE.

IN the good old days when the various branches of natural history were less specialised than now, and when everyone worthy of the name of naturalist could take an intelligent interest in the whole subject, the meetings of societies were a real and distinct incentive to progress. Members could appreciate most of the discussions, and they returned to their own special studies with suggestions from the results of kindred lines of research. Now, however, the whole condition of affairs has changed. Modern work in the various departments of biology and geology is so highly technical and special, that it is safe to say in most subjects not more than a score of investigators in the whole world can appreciate every bearing of each new contribution. Catholic societies, like the Royal Society, and the similarly named societies of Edinburgh and Dublin, the Literary and Philosophical Societies of Manchester, Liverpool, and Birmingham, and the Linnean Society of London, include among even their most distinguished members many who have not the faintest idea of the meaning and methods of the researches of many of their equally eminent fellow-members. No mind can now cover the whole field beyond the general elementary principles.

Under these changed circumstances, there are those who ask whether the meetings of such societies are any longer of value, beyond the casual conversations in the tea-room and friendly consultations in the library. Authors, indeed, are coming to care little for the meetings that they bore, and regard the societies merely as publishers less mean than most. When a specialist addresses an audience of

which not more than one or two members can appreciate a single sentence, is it not a waste of precious time for the others, and would they not be advancing science more effectually by spending a quiet hour instead in their own studies? The answer that would be given by many Fellows of the Linnean Society of London may be inferred from the way in which their meeting-room has been emptied by abstruse verbosity on more than one occasion of late.

On a recent occasion of this kind an author succeeded in practically emptying the room, leaving scarcely any Fellows beyond the officers and four members of council who felt it their duty to remain. The "reading" began abruptly with some thanks to botanists who had assisted the author. No reference whatever was made to the precise bearing of the new contribution on previous research in the same direction. No attempt was made to explain, to the Fellows who were not specialists on the group dealt with, the nature of the material, and yet no specialists were present. As the author proceeded, he picked out a casual sentence at irregular intervals from his systematic diagnoses, often not finishing the reading of the same even so far as the punctuation-mark; and the monotony of this procedure was only interrupted by an occasional rummage through a pile of dried specimens dumped down in absolute chaos on the table. Some of these the author explained, in a lucid interval, had been lent by the Director of Kew Gardens. We only hope that by some strange accident they escaped the pulverisation which to an onlooker seemed inevitable. On the board was arranged a series of diagrams, without the faintest indication as to what they represented, and the author only made a confused and incoherent reference to a small proportion of them. A map, duly exhibited to illustrate the distribution of the species, was not even referred to.

We have devoted so much space to this circumstantial recital, not because the instance was an isolated one calling for censure, but because the frequency of such performances requires that some vigorous and emphatic protest should be made, if our societies are to retain their old prestige and influence in promoting the communion of workers one with another. Nor have we mentioned the Linnean Society because matters are worse there than elsewhere; but surely if a paper is worthy of presenting to any society with such honorable traditions, the mode of presentation to the meeting is worth half-an-hour's preliminary consideration and arrangement. An author might at least have his illustrations in correct order for reference; he might explain the object of his communication and how it bears on the progress of that department of science to which it relates. He might deign to give his fellow-naturalists in other fields an outline of his methods that would be at least intelligible to them. If he is not prepared to do this, let him give his communication immediate burial in some special magazine which none but the specialists closely concerned are likely to see. The most technical contribution can, we maintain, be

made the subject of an intelligent brief address, if only its author will take a little pains; and we emphatically assert that it is the duty of the officers of all our Societies to prevent such disrespectful treatment of the Fellows or Members in meeting assembled, as that to which we have referred.

SIR HENRY HOWORTH.

A RECENT lively discussion in the House of Commons had at least one result of importance to scientific men: that philosophical and judicial critic, Mr. T. Healy, decided that Sir H. H. Howorth, though not much of a dynamiter, was "a distinguished geologist." This induced us to reconsider a few of the geological writings of our occasional correspondent. Two papers especially, recently published in the *Geological Magazine*, had not yet been otherwise disposed of. These discuss the Chalky Boulder-Clay of Eastern England, and the sands and gravels associated therewith, and their author absolutely repudiates the idea that any of these deposits are at all the result of ice-action. Such an opinion is unpopular with English geologists, and it is possible that scant notice will be taken of Sir Henry's summary of the objections to the orthodox explanation. Now, Sir Henry Howorth's geological work may be divided into two parts, one analytic and critical; the other constructive. The former attacks the theory of those whom he calls the "ultra-glacialists"; the latter enunciates a rival theory of his own. As a destructive critic Sir Henry is at his best; and his criticisms would appear to be so unanswerable that glacial geologists prefer to ignore them, and simply to ridicule the alternative theory that he proposes. Unfortunately for these folk, the papers before us contain no allusion to catastrophic deluges, but only a summary of objections to the view that in Pleistocene times a great ice-sheet deluged Eastern England and deposited the Chalky Boulder-Clay as a glacial moraine. There are remarks in these, as in other writings of our erudite humorist, that had been better omitted, for they weaken his case by exaggeration, and rouse opposition by over-emphasised heresy. But these clear statements of fact appear to us fatal to the ice-sheet theory: they show it to be inconsistent with the distribution and composition of the supposed glacial beds, and with the form and nature of the included pebbles and rock-fragments. Moreover, the extracts quoted from the writings of those who support the theory show that their classification of the East Anglian drifts is based on arbitrary distinctions. These papers by Sir Henry Howorth should be widely read, and ought to clear away much extravagant speculation.

THE ORGANISATION OF SCIENCE IN THE UNITED STATES.

Two important articles dealing with this subject have recently appeared in *Science*. The first, which is an editorial, appeared in the number for January 8, and urges a step that has been somewhat

seriously considered for at least twenty years, namely, the severance of the National Museum from the Smithsonian Institution, or at least the appointment of a director of the museum, who shall be other than the Assistant-Secretary of the Institution. The Smithsonian Institution, founded for "the increase and diffusion of knowledge among men," is not so much an American as a world institution, endowed by an Englishman for whom the Government of the United States acts as executor. The museum, on the other hand, as its name implies, is a national affair of the United States, having no logical connection with the Smithsonian, and constituting no organic part of it. While the Museum is now well established, and can readily be made directly responsible to Congress, the Smithsonian is hampered in its work by the necessity of applying to the Government for appropriations, and by the burden of administrative duties, which were never contemplated in its original scheme. A more personal reason is that a man like George Brown Goode, capable of serving both as a museum officer, and as Assistant-Secretary, and ultimately Secretary, of the great organisation, is not so easily found. The present situation, therefore, occurring as it does at the close of fifty years of the Institution's life, affords an excellent opportunity for reconsidering the relations of the Institution and the Museum.

The second article to which we would draw attention is by the Assistant-Secretary of Agriculture, Chas. W. Dabney, Jr.; it was prepared at the suggestion of the Hon. Gardiner G. Hubbard, and is in *Science* for January 15. It urges the formation of a national department of science, and points out how enormously the official scientific work of the United States is at present duplicated; for instance, "there are four hydrographic offices in as many departments, viz., the Hydrographic Office of the Navy, a similar office in the Coast Survey of the Treasury Department, the Division of Hydrography in the Geological Survey (Interior Department), which measures the rivers of the arid regions, and the Weather Bureau, which measures rivers and studies lake-currents." "In addition, the Fish Commission measures the waters of fishing-grounds and rivers, the Engineer Corps of the Army measures the Mississippi River, the lakes and harbors." A complete list of the scientific agencies is given in the article. "The only way" says Mr. Dabney, "to avoid duplication and waste of time and money, and to secure the proper coordination and cooperation, is to first bring all these bureaus together in one of the existing departments, or in a new department. When these bureaus have been thus brought together under the direction of one Secretary or executive head, the reorganisation will be comparatively easy. It should take place naturally and gradually in the course of ordinary business." "The policy should be to transfer the different scientific bureaus or surveys to one department, as opportunity offers, or as the Secretaries now having charge of them find it expedient to recommend it."

The great danger that all scientific institutions of this kind in America have to fight against, is the meddling of politicians. A real advance was made not long ago, when all those employed in the scientific bureaus were put in the classified service, corresponding to the Civil Service of Britain, and were thus no longer changed with the change of governments. Still there is always room for the enterprising Congressman, and there may be some to think that the National Museum is better off when responsible to an enlightened body like the Smithsonian Institution, than it would be when under the thumb of unsympathetic politicians. A great scientific department, too, would bring with it the inevitable red-tape; and it is doubtful whether the production of scientific work would be facilitated by a subdivision of the department according to means rather than ends, which is what Dr. Dabney seems to suggest. No one but a cheeseparing politician or a Treasury clerk would wish to see more offices like those of the Government Printers, the Stationery Office, or the Office of Works.

GEOGRAPHICAL DISTRIBUTION IN AMERICA.

THE boundaries of the life-areas of southern North America are discussed by Mr. C. H. Tyler Townsend in a paper "On the Bio-Geography of Mexico, Texas, New Mexico, and Arizona," lately contributed to the Texas Academy of Science. Mr. Townsend endeavours to lay down more precisely the limits of the regions and sub-regions marked out by Dr. Merriam. For this purpose he uses the distribution of plants as well as of animals. The Transition and even the Boreal Zone can be traced southwards in the higher mountain-ranges into Texas and Mexico, and at a lower level Upper Sonoran elements in the fauna and flora were noticed. The Neotropical belt is believed by Mr. Townsend to stretch considerably farther to the north along the west coast of Mexico than along the east; and he attributes this to the cold northerly winds which prevail at certain seasons on the Gulf coast, but never on the Pacific shore.

At the end of his paper Mr. Townsend gives a "Synopsis of the Life Divisions of America." He recognises five Regions, which he terms Boreal, Neotemperate, Neotropical, Austrotemperate, and South Boreal, the "Austrotemperate" including Chili and the Pampas, while the "South Boreal" is a circumpolar region to which Patagonia is transferred. What countries of the Eastern Hemisphere are included in it we are not told. This attempt to raise Patagonia, even with New Zealand perhaps thrown in, to the rank of a zoological region comparable to the Neotropical will hardly commend itself to students of the distribution, especially when the new region is saddled with so peculiar a name as "South (!) Boreal." And those naturalists who are inclined to follow Dr. Merriam in considering southern North America as worthy of regional rank will prefer the established name Sonoran to such a barbarous hybrid as "Neotemperate." We regret that Dr.

Merriam himself should have proposed "Austral" to supersede "Sonoran." To a resident in New York or Washington, "Austral" may be an appropriate term enough to indicate the region in question; but we owe so much to the American students of geographical distribution that we regret the more they do not seem to remember that their terminology should be appropriate for world-wide use.

A Remarkable Rodent.

In 1894, Mr. E. W. Nelson obtained from Mount Popocatepetl in Mexico, at and above a height of 3,000 metres, specimens of a small, short-eared, tailless rodent, which lives in the large saccaton grass there in considerable numbers, having habits and coloration like those of the common meadow mice of the mountains, with whose runways its own are intermingled. This rodent has lately been described by Dr. C. Hart Merriam under the name of *Romerolagus nelsoni* (*Proc. Biol. Soc. Washington*, x., pp. 169–174, Dec. 29, 1896), and has been referred by him to the Leporidæ. Its progression, however, is on all fours, in which respect, as well as in some important morphological features, it departs from the true leporine type. Although some Oriental species, of which Dr. Merriam is possibly not aware, show similar lack of agreement with the hitherto received diagnosis of the Leporidæ, yet, were it not for the high authority of Dr. Merriam, we should incline to place this Popocatepetl rodent with the Lagomyidæ. The following characters are admittedly those of that family: complete clavicles, short hind legs, absence of an external tail, and short ears. The skull, it is true, is said to be "much as in *Lepus* (subgenus *Sylvilagus*)." Nevertheless it departs from the leporine type in the depression of the brain-case, the posterior elongation of the jugals, the small size and absence anteriorly of the supraorbital processes — all, as before, characters in which it approaches the skull of Lagomyidæ. But, in the regrettable absence of information as to the dentition, specialists must for the present content themselves with recognising the form as one of unusual interest.

To the name given we have three distinct objections. First, we seem to recollect that the catalogue of the Mexican exhibit at the Chicago Exhibition contained the reproduction of a photograph of this same rodent, with the name "Lepus diazi, sp. nov," assigned to it by Ferrari-Perez. This has unfortunately been overlooked by Dr. Merriam. Next, we cannot approve of this combination of a modern personal name with a Greek noun: words like *Romerolagus*, *Leedsichthys*, *Agassizocrinus*, *Lichtensteinipicus*, *Uroskinnera*, and *Cookilaria* are the scorn of the cultured and the laughing-stock of the vulgar. Lastly, if this "new and remarkable" animal really has "an interest quite apart from that which attaches to most new discoveries," is it not a pity that the names proposed for it should be of an interest so limited and purely personal?

PALÆONTOLOGICAL PECCADILLOES.

WE are glad to find that American scientific men are beginning to protest against the curious methods of some of their fellow-countrymen. In the October number of the *Illinois Wesleyan Magazine*, Professor M. J. Elrod protests against some of the palæontological work published by the Indiana Geological Survey. "Diligent search," says he, "has failed to reveal a single palæontological specimen as belonging to the state of Indiana. The collectors have the pleasure of gathering them, attach their own names or have them attached, write descriptions, and the state foots the bill. So far as one is able to observe from reading these reports, no means whatever are taken to preserve these outside of private collections, and the hundreds of fossils described may at any time be carted out of the state, sold, or bartered away, and no one be the wiser. Yet we complain because legislators are not more liberal towards these scientific surveys! Evidently there are two sides to the question. We cannot wonder at the astonishment of our friends across the water at such proceedings. The wonder is that they have not long since been stopped."

Professor Elrod also protests against the absurd employment as specific names of the names of obscure individuals and villages. "The bugbear of science," says he, "is its array of names, often meaningless. When proper names are used they are still more meaningless." Certainly, when we find Professor Gorby's appended to thirteen different species in one volume, together with such monstrosities as "Kokomoensis" and "Boonevillensis," it is time, as Professor Elrod says, "to call a halt." As he points out, it is not only the name-givers that are to blame, but also the eponymous individuals whose "not objecting is generally taken as meaning that such a course is acceptable."

STRATA AS DIAGNOSTIC OF SPECIES.

THERE was once a man named William Smith who wrote a book called "Strata Identified by Organised Fossils." Hence he was known to his contemporaries as "Strata Smith," and to his successors as "the father of British geology." Since his day the principles laid down by him in England, and independently and almost synchronously by Cuvier and Alex. Brongniart in France, have been applied in greater and greater detail, so that there are now workers who, like Lapworth and Buckman, work out the succession of the fossils inch by inch through the stratified rocks, and find certain species characteristic of definite horizons over large areas. This minuteness of detail is achieved not merely by careful work in the field, but by accurate discrimination of species in the study. Lesser features are now recognised as diagnostic, and two forms that an older generation would have lumped together are now recognised, if not as distinct

species, at least as successive mutations of a species. This separation of mutations is akin to the detailed systematic work of the modern zoologist, who finds himself able to recognise geographical races and physiological varieties. But we must beware of reversing the process. One antelope is not to be separated from another merely because it lives in another part of the country; nor are two bees to bear different names only because they have a fancy for different flowers. Similarly one ammonite must not be labelled with a new name merely because it was found two inches or even two yards above one previously known. We know perfectly well that some forms of life can be subjected to varying conditions, or can live through many ages without becoming modified in structure. Moreover, to regard a parallel of latitude, or a particular stratum, as in itself a diagnostic character of a species, would be to upset the methods of stratigraphical geology, and the conclusions based on a study of geographical distribution. It would be an obvious logical fallacy, akin to putting the cart before the horse.

We have been induced to put these well-worn truths very clearly, because we have recently stumbled on two instances of the opposite, or shall we say the topsy-turvy, method. One instance was the peculiar remark of Dr. Wheelton Hind, criticised by our reviewer on p. 386 of vol. ix. To this author, however, we have no wish to allude further. It is more serious when we find a solid worker and acute writer like Mr. G. Dollfus, patronising the same heresy. In the *Revue Critique de Paléozoologie*, i, p. 40, when discussing the "Monograph of the Foraminifera of the Crag" by T. Rupert Jones and others, he writes: "Si la Paléontologie conserve sous un même nom spécifique des formes de tous les étages et de tous les pays, elle perd toute sa valeur . . . . elle crée pour les foraminifères une survie extraordinaire, toute spéciale, qui est une surprise et une anomalie dans le règne animal." Now Mr. Dollfus may believe that the Crag species of Foraminifera do differ in form or structure from their ancestors and descendants. But Messrs. Jones, Burrows, Sherborn, Millett, Holland, and Chapman believe they do not; while on the one hand they hold that local and temporary dwarfing is readily produced by lowering of temperature or diminution of food-supply, on the other hand, they believe that the Foraminifera are a group the majority of whose representatives have been subject to precisely similar conditions from very early times; they believe that they have an enormous range, and in that fact they see nothing extraordinary. Holding these views, are they really to give new names to their specimens merely because they come from the Crag and not from the London Clay? If Mr. Dollfus does not mean this, he has not expressed himself with the lucidity of his nation. If he does mean it, we can only suppose that he is a "special creation" man, a pre-Darwinian, and anti-Lamarckian, who has come out of the eighteenth century on the Time-machine of Mr. H. G. Wells, and doesn't know how to reverse its action.

THE MEANING OF A SPECIES.

IN the *Botanical Gazette* for December, 1896 (vol. xxii., pp. 454-462), Professor L. H. Bailey has a suggestive article on "The Philosophy of Species-making." Discarding Cuvier's conception of a species as "the reunion of individuals descended from one another, or from common parents, or from such as resemble them as closely as they resemble each other," no less than the older conception of a species as a unit of creation, he attempts to show that a species is a purely subjective idea, and consequently defines it as: "The unit in classification, designating an assemblage of organisms which, in the judgment of any writer, is so marked and so homogeneous that it can be conveniently spoken of as one thing."

This way of regarding a species involves inevitable consequences. First, extreme splitting: "I look with favour upon the tendency to make specific names for forms which have heretofore been regarded as well marked varieties." Second, arbitrariness: "Species are judgments," said Asa Gray; "I would not for a moment make it a test of a species that there should be no intergradient forms," says his pupil. Third, the exaltation of the species-spotter, the name-lover, the museum mind: "Species-division will be useful in proportion as it is founded upon obvious and easily ascertained attributes"; "it is not necessary, or even desirable, that we should search for obscure or anatomical characters with which to separate them. These characters belong to anatomy, physiology, embryology, and the like, not to taxonomy." No "physiological species" for Professor Bailey!

We quote this article because the views of a clever and experienced man on such a subject are always of interest. But we cannot agree to this separation of taxonomy from the other branches of biology, nor can we hold that expediency is everything. The last time we quoted an American botanist at length we were accused by an eminent though anonymous geologist of "endorsing" pernicious opinions, insulting to our supporters. Professor Bailey's views would perhaps please our critic; we must therefore explain that we do not endorse them.

A PINE AND ITS FUNGUS.

IN the same number of the *Botanical Gazette*, Mr. B. T. Galloway describes the action of a rust-fungus occurring on the leaves of the scrub or Jersey Pine (*Pinus Virginiana*) and causing the plant to cast its leaves. This result is known to follow the attacks of a number of fungi, and in this particular case Mr. Galloway has made an interesting series of observations on the course of events in the leaves affected. During the winter the leaves of the pines, in common with other evergreens, change colour, the dark green giving place to orange as the season advances. With the approach of spring the normal colour is resumed, growing brighter as conditions become more favourable. In early May those affected by the fungus (a

species of *Coleosporium*) may be recognised at some little distance by their pale yellow leaves and general thinness of growth. The colour is due to the effects of the fungus, which is confined almost entirely to or near the tips of the needles formed the previous season. The thinness of growth is owing to the lack of leaves, all except those of the previous year's growth having fallen prematurely.

The writer finds that the fungus is propagated by sporidia which appear on diseased leaves at the same time as the new leaves. The sporidia develop only in wet weather, and are washed or drop from those on which they are borne on to the young needles just showing their tips. The latter show no signs of infection for two or three months, and it requires twelve months to complete the development of the fungus. During a large part of this time it does not seriously interfere with the functions of its host-leaf. As soon, however, as it ruptures the tissues, loss of water is increased to about one-fifth above normal; the reserve water in the cells is gradually used up, loss of turgidity, and other physiological changes follow, resulting in the gradual death and casting of the leaves.

Experiments on the rate of loss of water from diseased and healthy parts of the same leaf show that, before the fungus has ruptured the cortical tissue, transpiration is less from the infected areas. This is found to be due to the permanent closing of the stomata, and may result in keeping the diseased parts alive longer than the healthy when a leaf or branch is removed from the tree.

Drunk and Incapable.

In a recent number of the *Journal of Botany*, J. L. Williams describes various observations made in North Wales on the intoxication of humble-bees by the honey of certain composite plants, and the relation of their consequent movements to the pollination of the flower. The bees were seen greedily sucking the honey from heads of flowers of *Scabiosa* and *Centaurea*, after which they were incapable of flying away, and rolled helplessly about in the flower-head, or fell to the ground. On recovering from their debauch, after a few minutes they immediately flew to another plant of the same species and began afresh. During their struggles the bees became covered with pollen, and on visiting a second plant were very effectual agents in impregnating its flowers with pollen from the one previously visited. Mr. Williams suggests that this method of cross-pollination may in time become normal. Hitherto its occurrence has been uncertain, and was only observed a second time after an interval of five years, when the writer was unable to obtain sufficient honey for analysis, so that the cause of intoxication is unknown. Whether or no advantages may accrue to the plant from thus 'drugging' the nectar which it offers to insect visitors, the comparative frequency of such effects cannot be denied. Nor are they confined to members of families like *Compositæ*

and *Dipsaceæ*, where the flowers are crowded into a head, and where alone there seems any chance of profit to the plant. On a warm sunny afternoon humble-bees may be found in large numbers drunk and incapable on tobacco plants; and we remember once counting as many as thirteen on one plant in the Botanic Garden at Cambridge.

EAST AFRICAN LEPIDOPTERA.

DR. W. J. HOLLAND has lately issued (*Proc. U.S. Nat. Mus.*, vol. xviii., pp. 741–767) a list of Lepidoptera collected by Messrs. Chanler and von Höhnel in the neighbourhood of Mount Kenia. While some of the species have a wide range over the whole Ethiopian Region, a much larger proportion show South African affinities, though there is no special relationship with the fauna of West Africa. So far as regards Lepidoptera, Dr. Holland is inclined to unite the South and East African sub-regions suggested for birds by Dr. Bowdler Sharpe (NATURAL SCIENCE, August, 1893.)

THE GREAT CARCINOLOGIST OF NORWAY.

To praise too much is invidious; in appreciating the carcinological work of G. O. Sars, it would also be difficult. In the amazing abundance and variety of his work, the clearness and accuracy of his descriptions, the dexterity and fulness of the accompanying illustrations, whoever may compete with him on one head or another, in all combined it may safely be said that he stands unrivalled. Within the tolerably well-marked confines of the crustacean group there is room for so much diversity of form that men who are recognised as specialists in regard to one division may be almost absolutely ignorant in regard to others. There is probably not a single one on which Professor Sars could not speak with authority; there are very few which he has not already handled with a master's touch. Though the Cumacea have in a sense been known for a century and a quarter, and even brought under the light of science more than half a century ago by the sagacious Kröyer, still it is to Sars that we owe the chief expansion of this still limited and curious order, and the clear understanding of its morphology. What he has done with pen and pencil would fully account for the time of an ordinary man, but Sars seeks his specimens not only in the cabinets of museums, but also in the depths and shallows of the ocean itself, securing thereby the advantage of seeing the creatures in the postures and colours of life. In many instances he has successfully studied the remarkable changes of form which some crustaceans undergo between leaving the egg and attaining to maturity. Not content with searching the fresh waters of his own country, we find him further engaged at home in raising Crustacea from the dried mud of other lands, and by this means able to comment on the life-history, and give figures and descriptions,

of many interesting Entomostraca from Australia, Algeria, New Zealand, and the Cape of Good Hope. While still occupied with the first volume (1890–1895) of his "Crustacea of Norway," comprising the Amphipoda, a standard work by itself sufficient to establish a solid reputation, he boldly undertook for the Imperial Academy of Sciences of St. Petersburg another large book on the Crustacea of the Caspian Sea (1893–1896).

Now is commenced the second volume of the "Crustacea of Norway," the first two parts consisting of 40 pages and 16 plates. These are concerned with the Apseudidæ and Tanaidæ, and of their value no better indication can be given than the incidental remark of a learned English colleague, who writes, "I have had the first number of Sars' Isopoda, and shall now try to work up the Tanaidæ I have on hand." Be it remembered that some of the species are less than a tenth or than a twentieth of an inch long, and then it will be understood why even experts gladly wait to see limbs and mouth-organs boldly and clearly figured by Sars before beginning to sort and name their own collections.

Just to keep up our reputation as destructive critics, we may point out that Professor Sars' keenness of vision for minute details enables or persuades him sometimes to distinguish genera and species which eyes more dim or more careless would willingly have done without. While giving almost everything else that could be desired, he whittles down synonymy nearly to the vanishing point. Doubtless this is owing in part to considerations of space, and in part to a humane desire to leave something, however humble, for others to do. One other feature may, by some of our most constant readers, be thought a more glaring fault than the rest. Those whose palates long for the acid of sharp criticism must go elsewhere than to Sars' writings, for his works will probably be searched in vain for one biting word about his fellow-workers.

It is highly honourable to the Bergen Museum to have undertaken the responsibility of issuing the "Crustacea of Norway," finding, as its notice says, "that, in the interest of zoological science, the further publication of this most valuable work ought not to be delayed." It is a national work, in the sense of being an honour to the nation which produces it, but Norwegian Crustacea are far from being confined to Norway, and, were it otherwise, the treatment of them by Sars would still make his volumes indispensable to every student of the group. This and all the important works of his later years have been given to the world in very satisfactory English. It would be pleasant to know what England has done for him, how many of our learned societies have enrolled him as a foreign member, and, of the millions of men and women here who are enthusiastic over the exploits of his brother-in-law, Nansen, how many individual persons have ever heard the name or given a thought to the labours of the brilliant and indefatigable naturalist, Georg Ossian Sars.

AN ANCIENT ARACHNID.

A WELCOME contribution to our knowledge of fossil Arachnida is a short "Note on Eophrynus," by Messrs. F. T. Howard and T. H. Thomas, published last year in the *Transactions* of the Cardiff Naturalists' Society (vol. xxviii.). The specimen forming the subject of the paper, and identified as *Eophrynus carbonis*, Woodw., was discovered in a coal-pit at Pentre in the Rhondda Valley. Unfortunately it consists merely of the lower side of the abdomen, no traces of the cephalothorax or appendages, about which there is still much to be learnt, being visible. Many interesting points of structure have, however, been preserved, and these have been clearly reproduced in the enlarged figures on the plate accompanying the paper.

For the following comment we are indebted to Mr. R. I. Pocock. "The authors with enviable confidence describe as stigmata five pairs of slits piercing the front margin of segments 2–6. If their interpretation be correct, the fact is one of far higher morphological importance than they seem to suspect. For no arachnid, recent or fossil, is known to possess more than four pairs of abdominal stigmata; and the recent species of Opiliones, the order to which *Eophrynus* and its allies, *Kreischeria* and *Anthracomartus*, appear to belong, are characterised by the presence of but a single pair placed upon the first free sternal plate of the abdomen. These considerations, coupled with the fact that *Eophrynus*, in spite of its great antiquity, is a specialized and not a primitive type, would make one inclined to suspend judgment as to the respiratory nature of the orifices in question.

"As for the name that Messrs. Howard and Thomas have assigned to Mr. O'Connor's specimen, I venture to think, though after all this is a small matter, that it is probably erroneous with regard both to genus and species. In the first place the structural differences between the abdomen of this fossil and that of *Eophrynus prestwichii* are too great, one would think, to permit the species in which they occur being rightly referred to the same genus; and in the second place Dr. Woodward's figure of the abdomen of *Eophrynus carbonis* represents this region as considerably longer than wide, whereas in the example from Pentre the abdomen, unless greatly expanded by crushing, must have been a good deal wider than long."

Caution in creating new genera and species is no doubt highly commendable, and no one will accuse palæontologists as a class with reluctance in projecting new names into literature. But in cases like the present where the characters of the fossil are clearly expressed, a wrong determination is apt to lead one's fellow-students into error.

PREHISTORIC MAN.

MR. JOHN WARD has collected together in the *Reliquary and Illustrated Archæologist* a compendious account of the exploration

of Rain's Cave, near Brassington, in Derbyshire. The excavation was of a thoroughly systematic nature and yielded an abundance of prehistoric remains, but nothing of an earlier date than neolithic scrapers and other implements, and bones of *Urus* and the wolf. The early discovery of a skeleton showed that the cave had finally been used as a cemetery.

In the same work there is a note on Carn Brea, in Cornwall. Some of these hut circles have recently yielded scrapers, arrow and spear heads, a diallage celt, and a small stone evidently used as a muller for grinding grain.

Has the Lamprey a Supra-renal Body?

We have received an interesting paper by W. E. Collinge and Swale Vincent on the structures in the cyclostome fishes hitherto regarded as the supra-renal bodies (*Anatomischer Anzeiger*, xii., pp. 232-241). The authors have made a most careful survey of the earlier literature on the subject, and the historical summary which they give should prove of great value to all students of ichthyotomy. The results of their investigations go to show that supra-renal bodies proper are not present in cyclostomes; that they first appear in the true fishes and acquire an increased importance in air-breathing vertebrates. They consider that the supra-renals are not degenerate structures "with a past," but that they have been independently evolved to fulfil certain functions which it remains for future physiologists to demonstrate. Whether anatomists generally will agree with the authors that there is not the homology between the pronephros of cyclostomes and the supra-renal bodies of gnathostomes that was maintained originally by Rathke and more recently by Weldon, time alone will show. Deeply rooted notions, whether based on incontestable observations or not, require very convincing arguments to overthrow them, and these latter are not furnished in the paper under consideration.

Sergestes.

An important contribution to our taxonomic knowledge of the marine Crustacea was made to the Zoological Society on December 1st, when the Rev. T. R. R. Stebbing communicated a paper by Dr. H. J. Hansen, of Copenhagen, on the species of the genus *Sergestes*. Dealing with the sixty hitherto described species, the author succeeded in reducing them to twenty, showing that previous observers had, from insufficient material, described as new, males and females, and young and adult stages of the same forms. His researches went so far as to render it very probable that there are trustworthy characters by which adults can be distinguished in future from immature forms, and it is to be hoped that the confusion that has arisen in the past will not be found a difficulty to future zoologists.

The Eggs o the Nautilus.

Our readers, who doubtless remember Dr. Arthur Willey's fascinating article "In the Home of the Nautilus" (Natural Science, vol. vi., p. 405, June, 1895) will join with us in congratulating him warmly on the success that he has at last gained. Professor Ray Lankester wrote thus to the *Times* (Feb. 8th):—". . . . Dr. Willey proceeded first to Ralum, in New Britain, where he spent a year trapping the nautilus in seventy fathoms of water and dredging in vain for its eggs. He then tried a station on the coast of New Guinea, where he was nearly drowned by the capsize of his small craft. After passing through New Caledonia he arrived last summer in Lifu, one of the Loyalty Islands, where nautilus can be captured in three fathoms depth only. Here he constructed a large submarine cage in which he kept specimens of nautilus, feeding them daily. On December 5th last his patient endeavours were rewarded. Some of the nautili had spawned in the cage, and thenceforward he was able to obtain abundant samples of the eggs. Each egg is as large as a grape, and is deposited separately by the mother nautilus. At present we have received but few furthur details from Dr. Willey, but he has doubtless by this time obtained the young in all stages of growth."

Evidence for the South African Committee.

We strongly commend to the gentlemen now appointed to enquire into South African affairs, a paper entitled "The Witwatersrand and the Revolt of the Uitlanders," written by Dr. George F. Becker, and reprinted from the *National Geographic Magazine* (Washington) for November, 1896. Dr. Becker, who is a well-known member of the United States Geological Survey, was in Johannesburg shortly after the Jameson Raid. He gives here a lucid and fair-minded account of the causes that led to that disaster, and of some of the occurrences at the time. He happened to be in a position favourable for obtaining information, and this he has sifted with scientific caution. We have rarely read an account of any impassioned dramatic struggle, even of one in a far-off antiquity, so sympathetic and yet so free from prejudice.

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I.

The Problem of Instinct.[1]

THERE is probably no subject in the whole range of biology, the study of which has been so universally neglected as Instinct. Both scientific and popular writers continually refer to it as if its nature and limitations were matter of common knowledge, and its facts so well established as to be almost above criticism. Yet when we ask how it is known that certain actions of man or animals are due to instinct and not to experience or imitation, we find an almost total absence of accurate observation or experiment, while hardly two writers are agreed as to the exact meaning of the term. It is only within the last quarter of a century that a few biologists have made any careful experiments on the phenomena presented by the actions of the higher animals, under such conditions as entirely to exclude the agency of imitation or of parental guidance; and although these experiments are as yet quite insufficient in quantity and far too limited in scope, having regard to the wide field covered by the actions and behaviour usually considered to be instinctive, yet the results reached are already very interesting, and are sufficient to show us that we need not despair of a complete solution of the problem, at all events as regards the higher animals.

One of the first English observers to attack the problem on the experimental method was the late Mr. Douglas Spalding, who in 1873, in *Macmillan's Magazine*, described a number of experiments on young chicks and ducklings, carefully blinded for the first few hours after birth. His conclusions were, that these young birds not only showed intuitive powers of walking, scratching, and pecking, but also possessed intuitive knowledge—or acted as if they possessed such knowledge—of various kinds. He asserted that they were afraid of bees and of the cry of a hawk, and that they intuitively knew the meaning of a hen's call-note and danger-signal when heard for the first time. These results were opposed to Mr. Spalding's preconceived ideas, and were therefore the more readily accepted, and have been frequently quoted as settling this point—the possession of instinctive knowledge as well as the power of co-ordinated movements of various kinds. Now, Professor Lloyd Morgan has repeated all

[1] HABIT AND INSTINCT, by C. Lloyd Morgan, F.G.S. Pp. 351. London: Edward Arnold, 1896. Price 16s.

these experiments many times and with a considerable variety of species, and, while confirming many of Mr. Spalding's observations and conclusions, has shown that those here referred to are erroneous. More important still, he has shown exactly where and why the conclusions arrived at are erroneous, and has thus afforded most valuable guidance to future experimenters in this interesting enquiry. Some examples of these corrections are the following.

Mr. Spalding noticed a difference in the behaviour of young chicks to flies and to bees, and concluded that they "gave evidence of instinctive fear of these sting-bearing insects." This, if true, would be very important, since it would show an intuitive perception of the dangerous character of a special insect, and if of one, presumably of all common dangerous or hurtful objects, antecedent to experience. But the whole series of observations made by Professor Morgan himself, as well as those made by other good observers, shows that they have no such perception of the qualities of objects. They pick up stones as well as grain, bits of red worsted as well as worms, gaudy-coloured inedible caterpillars as well as those which are edible. They do not recognise water till they have felt it, and they do not know that water is drinkable till contact with the beak sets up the nervous and muscular reactions of drinking. By a series of careful experiments Professor Lloyd Morgan shows that young chicks have no fear of bees as bees, but merely because they are large and unusual. They are equally suspicious of a large fly or beetle, and, though eating small worms greedily, are afraid of a large one. And when the chicks were a few days old, and were no longer afraid of large flies, they showed no fear even of wasps, when presented to them for the first time.

Very similar is the correction of Spalding's statements that his young chicks gave the danger-signal when a hawk was flying high overhead, and that a young turkey showed equal alarm when a young hawk, kept in a cupboard, uttered a shrill sound; whence he concluded that the fear of birds of prey, whether seen or heard, was instinctive. For it is now shown that *any* strange object or *any* unusual sound causes exactly similar alarm when first seen or heard by any kind of young birds, and Mr. Hudson, of La Plata fame, came to a similar conclusion. Other cases which have been thought to prove instinctive dread of enemies in various young animals are shown to be explicable on similar principles; any sight, or sound, or smell, very different from what they have been accustomed to, alarms them, and they learn what are really dangerous either through the actions of their parents or by their own personal experience.

But, though young birds and mammals do not possess instincts which enable them to discriminate between objects that may be useful and those that may be hurtful to them, they often possess the most wonderful acuteness of the senses and powers of co-ordinated muscular action, which enable them rapidly to acquire the knowledge that is essential to them. The young chick only a few hours out of the shell

walks and runs, and is able to pick up small objects in its beak, some being rejected and others swallowed. The young duck swims when put into the water, or when it accidentally walks into it, but it has no instinctive desire for it, and does not, as is often stated, run to it from a distance. Young dippers dive perfectly the first time they reach the water, and young swallows fly with great precision, avoiding obstacles almost as readily as do the old birds. With such congenital powers, and with an instinctive fear or suspicion of everything that is strange to them, they learn with marvellous rapidity; and having once found that a particular object is disagreeable or unfit for food they rarely require a second lesson, and thus in a few days accumulate a stock of experience, which, unless the process has been closely watched, may easily be set down to instinct.

About one-third part of Professor Lloyd Morgan's work is devoted to such experiments and observations on young birds and mammals as have now been indicated, and the amount of new and varied information here brought together is sufficiently large to form the basis of sound reasoning on the nature and limitations of the faculties involved; and perhaps no living biologist is better fitted to do this successfully than the author. In the series of chapters headed: "The Relation of Consciousness to Instinctive Behaviour," "Intelligence and the Acquisition of Habits," "Imitation," and "The Emotions in Relation to Instinct," we have a careful and interesting study of the physiological and psychological aspects of the facts that have been laid before us; a study which is in the highest degree instructive, and which will serve to guide future students of the subject both as to the interpretation of the facts already established, and as to the observations most needed for the elucidation of matters which are still unsettled. These chapters, however, are hardly suited for illustration or summary, and we will therefore pass on to those which deal with the alleged instincts of adult animals, and with some of the most disputed questions which now divide biologists; but, before doing so, it will be well to quote the author's definition of instinct, as well as the conclusions he has reached as to its nature.

At the end of the first chapter, which gives a popular sketch of the facts which demand explanation or verification, Professor Morgan says :—

"We may now sum up what has been advanced in the foregoing discussion, and say that, from the biological point of view, instincts are congenital, adaptive, and co-ordinated activities of relative complexity, and involving the behaviour of the organism as a whole. They are not characteristic of individuals as such, but are similarly performed by all like members of the same more or less restricted group, under circumstances which are either of frequent recurrence or are vitally essential to the continuance of the race. . . . They are to be distinguished from habits which owe their definiteness to individual acquisition and the repetition of individual performance."

And after having described the various actions of young birds

antecedent to experience, our author summarises some of the conclusions to be drawn from the observations as follows :—

"1. That which is congenitally definite as instinctive behaviour is essentially a motor response or train of motor responses. Mr. Herbert Spencer's description of instinct as compound reflex action is thus justified.

"2. These often show very accurate and nicely-adjusted congenital or hereditary co-ordinations.

"3. They are evoked by stimuli, the general type of which is fairly definite, and may, in some cases, be in response to particular objects. Of the latter possibility we have, however, but little satisfactory evidence.

"4. There does not seem to be any convincing evidence of inherited ideas or knowledge (as the term is popularly used); that is to say, the facts can be equally well explained on the view that what is inherited is of the nature of an organic response.

"5. Association of ideas is strong, and is rapidly formed as the result of individual acquisition.

"6. Acquired definiteness is built, through association, on the foundation of congenital responses, which are modified, under experience, to meet new circumstances.

"7. Acquired definiteness may pass, through frequent repetition, into more or less stereotyped habit."

Having thus given the author's standpoint and his main conclusions from a body of well-observed facts, we will pass on to his treatment of those activities of adult animals which are generally classed as instinctive.

In the chapter on "Some Habits and Instincts of the Pairing Season," the songs, dances, and displays of plumage by birds are described, and Professor Lloyd Morgan seems inclined to the conclusion that their great development indicates the action of that form of sexual selection termed "preferential mating." Some additional observations are quoted which support this view, but the final conclusion is that—"in all these matters further and fuller evidence from direct observation is to be desired."

The next chapter is on "Nest-building, Incubation, and Migration," and affords much matter for careful consideration. As to nest-building, I have always urged that careful experiments are required before we can accept as instinctive the building of a peculiar type of nest by each species of bird; and we find a few such recent experiments now adduced. But when we remember how such a careful experimenter as Spalding was led to wrong conclusions through not varying his experiments sufficiently, the few experiments yet made on nest-building under conditions by no means rigid and with results not described in sufficient detail, can hardly be accepted as settling the question. This is one of the problems that can only be finally settled by experiments tried on a large scale and with every precaution, and the results preserved for comparison and study; and if ever an experimental biological farm is established this subject of nidification

would form one of its most interesting and comparatively easy enquiries.

Passing over a very interesting discussion as to the habits of the cuckoo and their probable origin, and one hardly less interesting on the habit of the lapwing and many other birds of simulating injury to distract attention from nest or young, we pass on to the broader and more important subject of migration, which, however, is rather briefly treated. The evidence now accumulated seems to justify Professor Lloyd Morgan's conclusion, that while the migratory impulse is innate, yet, "the element of traditional guidance may be effectual, in the migration stream as a whole, in some way that we have hitherto been unable to observe." The chief obstacle to this view consists in the well known observations of Herr Gätke at Heligoland, that, during the autumn migration, in the case of the great majority of species, the young birds migrate earliest and alone, the adults following considerably later. But admitting, as everyone must, the accuracy of Herr Gätke's observations, does the conclusion necessarily follow? He himself assures us that the birds which rest on or pass within sight of Heligoland only form a fraction of the whole of the migrating hordes, most of them travelling by night at great altitudes, and very few passing within sight of the island, and of these few only, perhaps, one in ten thousand stopping to rest. The fact that young birds of many species are the first to visit Heligoland every year without exception, may possibly be explained by the fact that, while the older birds which lead the way travel high and go on without stopping, a large number of the young fly lower, and being either fatigued by the long unaccustomed flight or attracted by the sight of the land, descend to this elevated and fertile islet for rest and food. The late Mr. Seebohm, whose extensive journeys to Siberia and in various parts of Europe for the purpose of collecting and studying birds rendered him an authority on this subject, gives the early migration of young birds on the authority of the Heligoland observers, and does not support it by any observation of his own in the northern regions from which so many of the migrants come. In America, although some writers state that young birds migrate first in autumn, Mr. C. Hart Merriam, of the Department of Agriculture, tells us that this notion is "contrary to the experience of most leading American Ornithologists and to the information collected by the Committee on Migration of the American Ornithologists Union."[1] But if we reject the conclusion based upon the Heligoland facts as not necessarily following from them, we shall find that there is not much difficulty in forming a theory which accounts for the main phenomena, and the outlines of such a theory have been very well expressed by Mr. Seebohm himself at the end of the chapter on *Migration* in his "Geographical Distribution of the Charadriidæ," in the following passage:—

[1] Report on Bird Migration in the Mississippi Valley, by W. W. Cooke, p. 13, Footnote.

"The assemblage of migratory birds in large flocks, which in many cases wait for a favourable wind before they venture to cross wide stretches of sea, and consequently start altogether as soon as the weather is suitable, and arrive on the other side in enormous numbers or rushes; the keen sight of birds and their extraordinary memory for locality; the great variety of routes chosen, and the pertinacity with which each species keeps to its own route—these and many other facts all point in one direction. The desire to migrate is a hereditary impulse, to which the descendants of migratory birds are subject in spring and autumn, which has acquired a force almost, if not quite, as irresistible as the hereditary impulse to breed in the spring. On the other hand, the routes of migration have to be learned by individual experience. The theory that the knowledge of when and where to migrate is a mysterious gift of nature, the miraculous quality of which is attempted to be concealed under the semi-scientific term of *instinct*, is no longer tenable."

The views here expressed appear to me to harmonise well with the general conclusions as to the nature and limitations of instinct arrived at by Professor Lloyd Morgan, and they are enforced by some considerations which writers on this subject usually overlook. The numerous recorded facts of birds returning year after year to build in the same spots as in the preceding year, indicate that most of the spring migrants are old birds. Not only is this the general belief of observers, but it is rendered probable by the known longevity of most birds, and the obvious circumstance that those which have escaped the dangers of the double migration on the first occasion will be more likely to escape in each succeeding year, so long as health and strength continue. The fact that the breeding population of birds in any country does not increase year by year, but, though there are considerable fluctuations, remains on the average constant, proves that there must be an enormous destruction of the young birds, which certainly amount in number to several times as many as the old ones, and it seems probable that this destruction takes place during the two annual migrations, and more especially during the first one in autumn, when the young birds have had no practice in long continued flight and no experience of the dangers of the sea. If the birds of more than one season live on the average only four or five years more, it follows that only a very small percentage of the enormous annual progeny of young birds can survive to take their place. Hence it may well be that all those countless myriads of birds of the year that visit Heligoland are among the failures which, if they leave the island, perish in the waters. We know that enormous numbers *must* perish during each year, and where so likely as during that first attempt to traverse the North Sea? This is rendered almost certain by the recently issued Report of the British Association Committee on Bird Migration, in which it is stated that at the various periods of the great autumnal rushes at Heligoland, when countless thousands of birds pass over that island, no corresponding influx has been noticed on our east coasts during the four successive years that the two records have

been compared. Either then, these myriads of birds passed southward to Holland or flew out to sea and were mostly lost. As Herr Gätke does not mention any corresponding flights along the coasts of Holland and Belgium it is to be presumed that they have not been noticed, and we are almost forced to the conclusion that the greater part of these young birds, whose immense numbers at Heligoland excite so much astonishment, are really failures, and form a portion of those which are annually eliminated by the severe test of migration; and if this be so, much of the marvel supposed to attach to the successful migration of young birds disappears, since such "successful migration," except in the case of a small percentage, does not occur.

There is one alleged instinct of great popular interest which Mr. Lloyd Morgan does not deal with—the means by which many of our domestic animals, especially cats, dogs, and horses, find their way home under circumstances which seem to preclude any direct guidance by the senses. Narratives of the most marvellous character have been published, but, unfortunately, no systematic experiments appear to have been made, except a few by the late Mr. Romanes. These showed that a dog could exactly track its master's footsteps by scent, and it was shown that the scent was derived from his shoes, which must of course be full of perspiration and other emanations from the skin, because when he wore new shoes or those of another person his track could not be followed. In *Nature*, vol. vii., there is a considerable discussion of this subject, and many remarkable cases are narrated by various correspondents, but in none are all the data given for arriving at a rational conclusion on the question. I then wrote summarizing the discussion (*Nature*, vol. viii., p. 65), and suggested a series of experiments, which would alone give us any real information. My suggestion was that a dog whose antecedents were known should be taken by a circuitous route by rail and road to some spot where he had never been before, and should be there handed to some person he did not know, who should carry him a short distance, and on releasing him should keep him in sight, either on foot or horseback, till the animal returned home, noting carefully every movement and action. A moderate number of such experiments would settle the question of instinct or sense-observation, and it is to be hoped, now that a more intelligent interest is taken in the subject, such experiments will be made. I may add that, theoretically, any instinct of direction is almost inconceivable, because quadrupeds in a state of nature do not require such an instinct. They learn, step by step, the surroundings of their birth-place, extending their range, perhaps, year by year, but never requiring to go back over a country they have not previously traversed. In their case ordinary memory, assisted by very acute senses, would be all they would need. In the case of domestic animals returning home, we find that the recorded time elapsed varies from a few hours to several days or weeks, even when the distance can be easily traversed in a day. This shows that

an instinctive sense of direction cannot always be the agency employed; and, as in no one case is the exact route of the animal's return known, the assertion so often made, that a special sense of direction is required to explain the facts, cannot be justified.

Two very interesting chapters, near the end of the volume, are those entitled "Are Acquired Characters Inherited," and "Modification and Variation." In the first of these the bearing of the whole of the phenomena of instinct on the vexed question of inheritance is pointed out, and the conclusion is reached, that although there is little or no satisfactory evidence of the transmission of acquired modifications—that is of habits, or their effects on the organism, as opposed to instincts, yet there are many curious facts which seem to indicate some connection between congenital and acquired characters. What this connection is, the chapter on Modification and Variation attempts to show.

Modification of the individual by the environment, whether in the direction of structure or of habits, is universal and of considerable amount, and it is almost always, under the conditions, a beneficial modification. But every kind of beneficial modification is also being constantly effected through variation and natural selection, so that the beautifully perfect adaptations we see in nature are the result of a double process, being partly congenital, partly acquired. Acquired modification thus helps on congenital change by giving time for the necessary variations in many directions to be selected, and we have here another answer to the supposed difficulty as to the necessity of many coincident variations in order to bring about any effective advance of the organism. In one year favourable variations of one kind are selected and individual modifications in other directions enable them to be utilised; in Professor Lloyd Morgan's words—"Modification *as such* is not inherited, but is the condition under which congenital variations are favoured and given time to get a hold on the organism, and are thus enabled by degrees to reach the fully adaptive level." The same result will be produced by Professor Weismann's recent suggestion of "germinal selection," so that it now appears as if all the theoretical objections to the "adequacy of natural selection" have been theoretically answered.

Biologists owe a debt of gratitude to Professor Lloyd Morgan for this most interesting and suggestive volume. It exhibits all the clearness and philosophical acumen which characterise the writings of the author, and although in his desire to be impartial he has sometimes suggested difficulties which are more apparent than real, yet the work is on the whole an admirable introduction to the study of a most important and fascinating branch of biology, now for the first time based upon a substantial foundation of carefully observed facts and logical induction from them.

ALFRED R. WALLACE.

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II.

Fresh-water Biological Stations: Europe's Example.

MR. SCOURFIELD'S plea for a fresh-water biological station in England (NAT. SCI., vol. x., p. 17, Jan., 1897) gave me much pleasure. England is very late in making this proposal, and all biologists working on lakes have often asked why the English lake-district remains unexplored in this respect.

Every day fresh-water biology assumes greater importance, and in a country so far from the sea as Bohemia, we naturally came to the conclusion that the study of our fresh waters was our first duty. Last summer I delivered a course of lectures drawing attention to the variety and interest of the life-groups in more than thirty kinds of Bohemian fresh waters, each with its own special fauna and flora: springs, mountain brooks, mountain rivers, rivers of the plain, back-waters of large rivers, ponds, lakes, bogs, small pools with *Apus*, snow-tarns with *Branchipus*, etc. Each of these kinds of water varies in its own fauna with the seasons of the year, and also from year to year according as rain and sunshine also vary. Here is work for a century.

Besides America, as you yourselves have mentioned, Russia also is beginning to support the biological study of fresh water. A fixed laboratory is to be established in connection with an agricultural institution, while both moveable and smaller fixed stations will be founded in different parts of Russia. The sum of 73,000 roubles has been placed at their disposal. Even if these institutions were almost to confine their attention to fish, biology would gain much.

The sum of £500, which is suggested as enough to start such a biological station in England, is none too much. The building for our moveable station was presented to the committee by a friend of mine, and cost him £70. With all its internal fittings, it now has a value of £200, yet everything is very humble, and the want of better instruments strongly felt. The annual working expenses of three investigators amount to £40, their work itself being given freely. Nevertheless, we have just finished the examination of two lakes in the Böhmerwald, the report on them being now in the press; and the station has been transferred to Podiebrad in the middle of Bohemia, for the investigation of the river Elbe. At the fixed station, I may

add, we have examined the plankton of the pond near Biechovic every fortnight, paying special attention to infusorians and rotifers.

Last summer I visited two biological stations in Germany, and published in *Mittheilungen des Oesterreichischen Fischerei-Vereines* (No. 65, Wien, 1896), a paper entitled, "Eine Studienreise in Angelegenheiten der Fischerei." As the information may assist your proposal, and is not likely to be accessible to your readers, I venture on an extract or two.

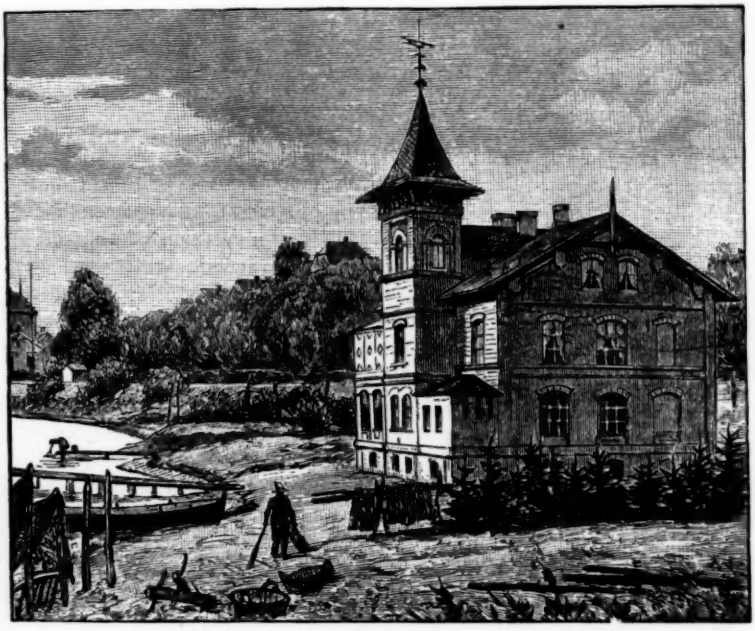
The biological and fishery station on the Müggelsee near Berlin is close to the great water-works of Friedrichshagen, built at a cost of £1,000,000 to supply the capital with water. These waterworks

THE BIOLOGICAL AND FISHERY STATION ON THE MÜGGELSEE.

supply the station gratis with a definite quantity of water; not, however, with enough for the experimental basin, to which water is to be pumped from the lake by a motor next year.

"Professor Frenzel was waiting for us, and was kind enough to explain the arrangement of the station in detail. On the sandy, slightly shelving shore of the lake, in a fir-wood, stands a small one-storeyed cottage with four chambers, two workrooms, an aquarium-room, and the servant's lodging. The workrooms have each about the size of our moveable stations, but suffice for the work of an unassuming naturalist. The tanks in the aquarium are of cement, rendered harmless, when finished, by glowing charcoal; many smaller

ones are of glass. The experiment-basins in front of the buildings were for the most part empty, since the Government supply was not enough to fill them all. A petroleum-boat is at the service of the station. During the last three years this institution has been granted 27,806 marks from the German Fisheries Union. A detailed report on the arrangement and work carried on was published by Dr. Frenzel in the *Zeitschrift für Fischerei*, Heft I, 1895; separate copies of it may be obtained. Of late, importance has been attached to attracting young people, who come here from the Agricultural College in Berlin, and from the University, and obtain an insight into the fauna of the lake.



THE BIOLOGICAL STATION ON THE PLÖNERSEE.

"The Müggelsee Station is undoubtedly a good beginning, and I received the impression that Germany was ready to do much more for the study of fresh-water biology, so soon as the results there obtained should receive universal recognition.

"The Plönersee lies in a delightfully picturesque district, reached in about two hours from Lübeck, and on its shore, in the town of Plön itself, is the biological station, where for the first time in Germany the scientific investigation of fresh water found a decent shelter. I was kindly received by the Director, Dr. Zacharias, and in the short time at my disposal I obtained, through his guidance, an idea of the arrangements of the station. The building contains a

number of fairly large rooms, easily accessible from a corridor between them. Besides the laboratory of the Director, and of an assistant, there is a larger apartment for researchers visiting the station, then a room for the nets, and a library. On the ground-floor are aquaria, ventilated by a petroleum motor of two-horse power.

"The catches are exactly measured, and then counted as individuals, after Professor Hensen's method,—a very tiresome and lengthy proceeding. For our work in Bohemia, where we have to do with shallow water, the quality of which and the plankton vary suddenly and often, this method is not to be recommended; moreover, we obtain sufficient knowledge of the distribution and composition of the plankton by horizontal dredgings of 100 metres, going metre by metre.

"The results of Dr. Zacharias and others working at the laboratory are published in the *Forschungsbericht*, of which three volumes have appeared up till now.

"The establishment of the institution, as well as its direction, are still of a provisional character, which hardly conduces to prosperity. When we consider the happy progress that has been made in the investigation of fresh water by the institution of the three stations in Plön, on the Müggelsee, and in Trachenberg, it is strange that opinions as to their value should be so diverse. People underestimate the problems set before such institutions, and forget that the individuals who are to cope with them, must first be discovered, that they must make systematic studies throughout many years, and above all, that they should be placed in a position untroubled by anxiety as to their future, and not compelled to supplement by other undertakings the insufficiency of their material situation."

I hope that you who in England are supporting this movement for a fresh-water station, will be able to convince people of the great practical, as well as scientific value, of such an institution, and that we on the Continent may soon hear that your wealthy country has done her duty by fresh-water biology.

ANTON FRITSCH.

Prague.

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III.

The Light-Sensations of Eyeless Animals.

THE fact that certain skins are sensitive to light has long been known. Most of us are familiar with the case of the earthworms, whose sudden withdrawal into their holes on the approach of a lighted candle was observed and described by Darwin. The earthworms have no eyes, and there is no other apparent explanation of this reaction but that the skin itself is sensitive to the light-rays. It is also well known that this is not the case with all skins—our own, for instance—and that, in the vast majority of animals, there is only one organ, viz., the eye, which is sensitive to variations of light intensity.

The assumption is justifiable that if we could only explain the mechanism of this diffuse light-sensation of the skin, usually called, not very happily, the "dermatoptic" function, we should be in a fair way towards understanding the mechanism of the exquisite sensitiveness of our eyes. It is, indeed, open to anyone to assume that this diffuse sensitiveness was once universally present, but was lost on the development of eyes, these specialised organs rendering the less efficient function unnecessary; and that eyes first arose merely as localised areas, in which this sensitiveness to light was specially concentrated.

It is quite possible that this may have been the case: it is a fair hypothesis; but, on the other hand, the facts seem to show that the dermatoptic function is not necessarily a primitive condition retained, but may also be secondarily acquired. It is found, for instance, in isolated members of different animal groups, and nearly always in adaptation to some special manner of life; this favours the idea that it has been secondarily developed. The ancestors of the earthworm, for instance, were probably more free-living chætopods, and, as such, almost certainly possessed eyes, which were lost in the earthworms owing to their burrowing manner of life. I know of no record of any modern chætopod possessing the dermatoptic function.

With regard to the snails, which possess both eyes and, to a slight degree, the diffuse sense, shrinking at a passing shadow even when the eye-bearing tentacles are cut off, we may, on first thoughts, suppose the diffuse function either to have persisted along with the specialised, but not very highly developed, eyes, or to have been

secondarily acquired. But the fact that the nearly related slugs show no such sensitiveness to light seems to indicate that the second hypothesis is more likely to be correct. On the other hand, in the case of blinded newts, which select the darker places in which to lurk, as if their skins were in some way sensitive to light, this might be thought more likely to be the primitive function retained, otherwise it is not easy to see why it should have been required in addition to eyes. The reported sensitiveness to light of the blind cave-dweller, *Proteus anguineus*, is a quite intelligible adaptation to the needs of the animal.

Here, then, we are brought face to face with a very fascinating problem. The skins of animals may possess a sensitiveness to light which must in some way be correlated with the development of eyes. Have we any clue which will explain the facts, and also the connection between the diffuse light-sensation and the specialised light-sensation of our eyes? All who are interested in this—and who is not?—will naturally welcome any book dealing specially with this subject. Probably all that can be said about it from the generally accepted point of view can be found in Dr. Willibald Nagel's 120 pages, published by Gustav Fischer, of Jena, under the title of "Der Lichtsinn augenloser Tiere." Excellent as Dr. Nagel's treatment of the subject is, there is, as we shall see later on, a factor in the problem which Dr. Nagel is not alone in too lightly dismissing, though due attention to it would have brought him nearer to some positive and satisfactory conclusion.

The first part of the work is a reprint of a lecture under the somewhat startling title of "Seeing without Eyes" (Sehen ohne Augen). The second part contains a description of experiments made by the author in the Naples Aquarium and elsewhere on the reactions, chiefly of molluscs, to sudden positive or negative variations of light-intensity. The third part consists of five short appendices or notes, expanding certain subjects which had been too briefly dealt with in the lecture; and the volume concludes with a useful bibliography of treatises on the physiology of sensation in general, and of sight in particular.

In addition to the better known examples of the dermatoptic function above mentioned, all of which are cited in Dr. Nagel's book, we have the phenomena especially investigated and described by the author himself. These require a brief notice.

First of all, then, the author established beyond doubt that, apart from the stimulus of very bright lights, it is the sudden changes in the light-intensity to which skins endowed with the dermatoptic function react, and further, that some animals react to sudden increase of light, and others to sudden diminution. That the oyster, for instance, should react to a sudden diminution of light had been denied by Rawitz, more or less on theoretical grounds. The denial and the objections are conclusively met by the author, the former

experimentally, by showing that if a number of oysters kept in a vessel are found open, they will shut simultaneously the moment a dark object comes between them and the light. While the shutting of a single oyster on the passing of a shadow might be a mere coincidence, the simultaneous clapping to of a number of individuals excludes this possibility.

The animal that gave the best reactions among the molluscs was *Psammobia vespertina*, a bivalve which, as the name implies, lives in sand. The siphons of this animal protrude from the sand as delicate, whitish, transparent tubes, without a particle of pigment. If these siphons are suddenly illuminated, care being taken to avoid any mechanical stimulation, they contract. It is important to note however, that there is a short latent period of about a second. The brighter the light, the greater the contraction.

If a number of *Psammobia* are placed in a glass with plenty of sand and carried into the direct sunlight, they bury themselves instantaneously. If there is no sand in the glass, they flit about in the water until exhausted. But while very bright light violently stimulates these animals, it was observed that, with moderate illumination, it was not the light itself, but the sudden change from less to greater which caused the reaction; a moderate light appears to be that to which they are accustomed.

Having established the extreme sensitiveness of the siphons of *Psammobia* to sudden increases of light, the experiments were varied. Sudden diminutions were tried, but without other result than a slight lateral movement. *Psammobia* therefore belongs to the group which only responds to sudden increase of light, and this, it is found, contrasts with another mixed group of animals which only respond to sudden diminutions of light, and are not stimulated by its sudden increase. These two different and opposite kinds of reaction are called light-sensation and shadow-sensation.

Again varying the experiments, Dr. Nagel found that illumination with lights of different wave-lengths showed that all the visible spectrum except the reds led to reaction. This effect is interesting and important, and we shall have to refer to it again.

It is not necessary to follow all the experiments, or to repeat a list of the recorded cases referred to by the author. The sudden closing of the sessile brachiopod *Thecidium*, on the passing of a shadow, is not given. I mention it here as a further indication of the secondary acquirement of the dermatoptic function. This animal has a method of life not unlike that of the oyster, although there can certainly be no close genetic relationship between them. In both cases the animals are exposed when the valves are open, and in both cases the lids clap to on the passing of a shadow.

A generalisation of interest to biologists must be noted in passing. All the animals which responded to the sudden diminution of light were such as inhabited strong shells, as in the cases just mentioned,

or lived in tubes into which they could withdraw, their times of exposure being when feeding. The significance of this the author, no doubt correctly, points out. Warning of the approach of an enemy would in most cases be first given by its shadow. Hence this "Schattenempfindung" is most probably protective. On the other hand, the sensitiveness to light-increase is most prominent in animals which live in sand or mud, from which they occasionally and, as it were, shyly emerge. Their sensitiveness to light serves to keep them near their retreats. *Amphioxus* is a good example of this. It lives in sand, and is violently agitated by light if it cannot bury itself. It was originally thought that the sensitiveness to light of this animal might be due to the pigment spot at the anterior end of the nerve cord, but "headless" specimens are equally shy of light. This appears to limit the seat of the sensation to the skin.

These very variations of the function, in more or less evident adaptation to the biological needs of the animals, are, it seems to me, a further indication of its secondary origin, and we are led to believe that, if need be, any skin could acquire the capacity of reacting, either to sudden increases or to sudden diminutions of light-intensity if its life-condition required it. I say we are led to believe this simply because so many different animals have developed it, and quite apart from any theory as to the physiological principles involved; we shall return to these latter later on.

We noted above that Rawitz was rather inclined to doubt the existence of a "Schattenempfindung," his reason being, that if light were a stimulus, the negation of light could not be so at the same time. Dr. Nagel discusses the point in one of his appendices. He rightly points out that the fallacy in this argument lies in supposing that the light-waves as such are felt, *i.e.*, give rise to some corresponding vibrations in the nerves. This is certainly not the case, and all we can rightly assume is that certain potential energies exist within the living substance, and that these are brought into play by the action of light, the resulting molecular movements having no necessary likeness to light-waves. From this point of view, changes in the molecular or atomic movements within the living substance, so far as we can see, could be produced as well by sudden illumination as by sudden shadows. Such changes are all, then, that we require for the production of the sensation. Further, Dr. Nagel's answer to Rawitz might have been still more complete if he had pointed out that the sudden diminution of light most certainly causes a distinct sensation with us. A sudden shadow always startles us. In view of the justifiable correlation between the specialised organ for recording variations in light-intensity, and the skin possessed of the dermatoptic function, there seems to be no reason why, in such essential matters, we cannot assume that what is true of the one will be true of the other also. Indeed, as above noted, Dr. Nagel has experimentally established this shadow-sensation in oysters.

After having established the facts concerning the sensitiveness to light of the skin in certain animals, the next question that arises is: Are there any specific sense-organs functionally associated with this sensation? Its answer, contained in Appendix 3, p. 98, is: no such organs can be found. The skin is richly provided with simple sensory cells, and the author is driven to assume either that there are subtle differences, not evident in their microscopic appearances, between these cells, some being tactile, others sensitive to light and so on, or that one and the same apparently simple sensory cell may serve for different sensations. Dr. Nagel is an advocate of the latter hypothesis. The infusorian is a simple cell and is capable apparently of very diverse sensations; at least it reacts to many different stimuli. And, again, any cell has at least two different and opposite reactions, viz., expansion and contraction, and these, with their degrees, might make it possible for one and the same cell to be the end-organ of more than one sensation.

We may then briefly sum up Dr. Nagel's conclusions. The whole skin is a possible retina; cells sensitive to light are there, and the nerves connecting these cells with the central nervous system; all that is wanted to form an eye proper is a dioptric apparatus, *i.e.*, primarily, a lens to throw an image on the skin. Distinct vision of the external world would result as soon as there were a sufficient number of the sensory cells to give a connected translation of the image with its various lights and shades into sensation.

Beyond these suggestions as to the probable explanation of this curious function and of its relation to specialised vision, Dr. Nagel does not go, and indeed it does not seem possible to go further along recognised lines. As above stated, however, Dr. Nagel follows in the beaten track, and practically neglects one important factor in the problem. I refer to the pigment.

The complete absence of the pigment in the siphons of *Psammobia* already noted is especially emphasised by Dr. Nagel, as tending to strengthen the ordinary view that pigment is not necessary to sight, a fact which was already known from the absence of pigment in the eyes of albinos. Nevertheless, Dr. Nagel, who may be taken to be expressing the generally accepted doctrine, is obliged to admit that the pigment has some very intimate association with light-sensation. It is almost univerally present in eyes, being absent only in albinos, which must be considered as abnormal (Missbildungen). What this relationship is no one has hitherto been able to explain. And on this subject Dr. Nagel confines himself to the oft repeated suggestion, that the pigment forms sheaths round the sensory cells. In this way, the dazzling of the light is prevented, and each end organ is illuminated by itself, the distinctness of the image being thus assured. I should not, of course, wish to deny that this ensheathing of the rods and cones for the isolation of the end-organs is one of the functions of the pigment. There are certain facts, however, which

render it somewhat doubtful. The pigment in the eye, for instance, is very granular, and granules, especially if they are, comparatively speaking, large, are not the best materials for a light-proof sheath. And again, the more perfect the dioptric apparatus, the less necessity is there for this ensheathing of the nerve endings, inasmuch as the rays are already arranged in definite small-angled pencils, whose directions practically coincide with the long axes of the nerve endings. Yet we do not find that pigment is less developed in the highest eyes. Indeed, intense pigment is found even in the eyes of mammals whose skins are pure white in every other part of the body.

We have then, it appears to me, to look elsewhere for the chief function of the pigment, the universal presence of which in normal eyes makes it highly probable that that function is one of supreme importance in the physiology of sight.

Let us see first what are some of the recognised effects of light on the skin. One well-known effect is that which is called sunburning. This browning of the skin is due to the movement under the influence of light of pigmented granules between and into the cells of the skin. In the case of the Vertebrata a certain amount of pigmented matter is found in the cutis, and this seems to travel outwards towards the light. Whether the light helps in any way to produce more pigment, perhaps by increasing the metabolism, is unknown. Certain it is that pigment frequently appears where specially active metabolism incidental to growth is going on. One might mention as examples the case of the regrowing of the lizard's tail and the rapid multiplication of cells in certain cancerous growths; in both these cases, pigmented matter is very abundantly produced.

Our special point here, however, is that this pigmented matter invades the epidermis under the action of light. It forces its way between the innermost cylindrical cells and eventually reaches the outermost cells where, excepting when very abundant, it disappears, becoming incorporated, with loss of colour, in the dying horny cells which form the outermost protection of our body. So active is this process, indeed, that if the light contains a large proportion of rays from the violet end of the spectrum, the sunburning may be very pronounced; excessive horn-cells are produced and the outermost peel off.

Here, then, we have a very distinct effect, involving the movement of granules between and within the epithelial cells, taking place in the skin under the influence of light. It is surely not difficult to believe that this process might, if required, come under the cognizance of the ordinary sensory cells of the skin. Judging from what we now know of the origin of gustatory and auditory sensory areas out of epithelial cells, which indirectly stimulate the nerve fibres in contact with them, there is surely no insuperable obstacle to our believing that other epithelial cells, stimulated by the movement of pigmented granules set in motion by the action of light either within

their bodies or between them, also indirectly and mechanically stimulate the indifferent nerve cells or fibres in contact with them. It seems to me clear that the specialisation of this process might give rise both to the dermatoptic function and to eyes.

In the first place, it must be noted that the retinal cells, in the great majority of eyes, are modified epithelial cells. As such it is quite possible that they are not themselves directly continuous with nerve fibrils; the nerve fibrils may be merely associated with the retinal cells. It would then be these latter that are stimulated by light, and they in their turn stimulate the associated nerve fibrils. This possibility is admitted in the last edition of Quain's Anatomy, vol. iii., pt. iii., p. 155; 1894. It is now generally acknowledged, as there noted, that this indirect stimulation of the nerves by modified epithelial cells obtains in the specific organs of touch, taste and hearing. It has not, however, been established either for sight or smell, although, as above stated, there are reasons for believing that it holds good for the former. In view of this indirect stimulation of the nerves in three out of our five senses, much of the discussion as to the possibility of undifferentiated sensory cells responding to different stimuli is, at least in the present instance, not quite up to date. At any rate it seems to me that the simpler problem should come first; what are the changes produced in the epithelium by light? Now it is well known that the epithelial cells are influenced in a striking way by light, viz., in the movements within and between them of the pigmented granules. Cannot this fact supply us with a clue to the understanding of the sense of light, whether diffuse or concentrated in eyes?

When we turn to the eye, we find that one of the few effects of light, about which we have no doubt, is that the pigmented granules move backwards and forwards in enormous quantities between the prolonged outer ends, the rods and cones, of the retinal cells. This process must surely be regarded as a specialisation of the ordinary invasion of the epidermis by pigmented matter under the action of light. For not only is the eye a specialised portion of the epidermis, but in both the eye and in the epidermis the pigment travels outwards under the action of light. It seems to me indeed impossible to avoid the conclusion that eyes are only specialised organs for the utilisation of these movements of the pigment under the action of light in the interests of sensation. For if the stimulation of epithelial cells by waves of sound can be communicated to the associated nerves, awakening the sensation called hearing, there is no reason that I can see why the stimulation of the retinal cells by the movement of granules against their surfaces or within their bodies should not also be communicated to the nerves, awakening the sensation called sight.

As I have elsewhere maintained, eyes arising by the stimulation of the ordinary sensory nerves of the skin indirectly by the epithelial

cells, which are themselves directly stimulated by the movements of granules within or in contact with them, would naturally appear at those spots which are most frequently and brilliantly illuminated. It would be to such parts that the pigment would be most drawn by the light. That is not all. The very excess of pigmented matter brought to the surface by the light would supply the material for a dioptric apparatus. (See further *Ann. Mag. Nat. Hist.*, Feb. 7th, 1896, and *Quart. Journ. Micro. Sci.*, Dec., 1896.)

Armed with this clue to the understanding of light-sensations supplied us by the pigment, we turn to those cases in which no pigmented matter can be detected, *e.g.*, in the eyes of albinos and in the semi-transparent siphons of *Psammobia*, both of which are stimulated by light. The clue in no way fails us. The essential point involved in it was the movement of granules. While it is possible that deeply-pigmented granules move more actively under the action of light than unpigmented granules, there is no reason whatever to suppose that it is only the pigmented granules which are affected by the stimulus.

As a matter of fact, pigmented granules are of all shades of colour, from deep blackish brown to light reddish and yellowish browns. We have only to assume, then, that in the cases where no pigment can be detected, granules are nevertheless present, which are set in motion by the light, or, in the case of the shadow-sensation, brought to a standstill by the sudden withdrawal of the light, and that the epithelial cells stimulated by this motion or sudden cessation of motion in their turn stimulate the associated nerve-cells or fibrils. I may, indeed, be allowed to state in advance that, among many other retinas which I am now investigating, that of the albino rabbit shows clearly that this supposition is partly, if not entirely, correct. Not only is coarsely granular matter present in the chromatophoral cells in such eyes, but, further, this matter is frequently found massed high up between the retinal rods, just like the pigment granules in normal eyes. While I have no distinct proof that this matter finds its way between the rods as a direct effect of the action of light, it would be hazardous to deny the probability of this having been the case.

I think, then, it is abundantly clear that the pigment, in its broadest sense, meaning pigmented granules, cannot be dismissed in the way it usually is, as unessential to light-sensation, merely because in certain abnormal eyes there is no colouring matter. On the contrary, it seems to me that if, instead of turning away from the functions of pigment as too problematical to be worth considering, as Dr. Nagel has done in his little book, he had concentrated his attention upon them, his interesting facts and able discussion of them might have been carried a step or two further.

H. M. BERNARD.

Streatham, S.W.

565.3309

IV.

The Literature of Fossil Ostracods.

THE common *Cypris* of our rivers, ponds and ditches, or the various genera, both freshwater and marine, belonging to the same class of animals, have had representatives or close allies in every geological period from the Cambrian downwards. It is the purpose of this sketch to bring before the readers of NATURAL SCIENCE a general idea of the amount of work already done on this group, and the probably great amount of ostracod material preserved in the rocks beneath us.

Up to the year 1855, valves of Ostracoda from Palæozoic rocks had been described here and there, with other fossils, but little serious attempt had been made to study any of the groups as a whole, or to classify such forms as had received the scanty technical attention given by the collectors of these fossils. In 1855 Professor Rupert Jones, then assistant secretary of the Geological Society, commenced a series of papers on the subject in the *Annals and Magazine of Natural History*, a series which has now reached number xxxii., and has provided material wherewith to study and classify the whole known group. In the elucidation of the various forms as they came to hand, Professor Jones had, from time to time, the invaluable assistance of Harvey B. Holl and James W. Kirkby, and these authors between them have published a vast amount of information on a most difficult group.

The Palæozoic Ostracoda present so many anomalies, that comparison with recent forms is often impossible, and deductions that were at first drawn have had to be abandoned as fresh material has come to hand. The singular protuberances found in the carapaces of the genus *Beyrichia*, with their hundred and one modifications, and the still more remarkable projecting spines of *Æchmina*, seem to baffle suggestion as to their use and meaning.

An illustrated account of some of these early forms appeared in 1869 in a paper, now very scarce, published by the Geologists' Association of London. This gives the student a very good idea of the various modifications undergone by the ostracodal carapace. In 1886 (*Quart. Journ. Geol. Soc.*) Jones and Kirkby gave a synopsis of all British "Carboniferous" forms, arranged to show their zonal and geographical distribution, and further showed that while the genera provided a more or less safe guide to stratigraphy, the species

were fairly represented throughout the whole series. In a paper that has recently appeared (*Sci. Trans. R. Dublin Society*), Jones and Kirkby deal with the Carboniferous Ostracoda of Ireland, and in a table at the end of the work have given their geographical distribution in that island. Much, however, remains to be done before we can correlate the species with those of Britain and the Continent, and learn the true value of the forms, or their value to stratigraphy. A similar paper from these authors, dealing with the Silurian, Ordovician, and Cambrian forms, would be of much service, and would considerably advance the subject.

Notes on Devonian Ostracoda will be found scattered throughout the series of papers in the *Annals* above mentioned, and it might be worth some student's while to draw up for the whole Palæozoic series such a table as is suggested.

On Mesozoic forms many papers have appeared, the chief of which are those by Jones (*Quart. Journ. Geol. Soc.*, 1884), on the Richmond Well; by Jones and Sherborn (*Proc. Bath Field Club*, 1888), on the Fuller's Earth; by Jones (*Geol. Mag.*, 1878, p. 103, &c., and 1888, p. 534, &c., and *Quart. Journ. Geol. Soc.*, 1886), on the Purbeck and Wealden; also Jones and Hinde (*Palæont. Soc.*, 1890) on the Cretaceous in general. This last monograph was a second and improved edition, so to speak, of Professor Jones' original monograph of 1849, and, besides revising old work, brought the subject right up to date; a separate paper (*Geol. Mag.*, 1893) by Frederick Chapman and Sherborn, dealing with the Gault of Folkestone zone by zone, completed the history of the Cretaceous forms as known up to that time. Among these Mesozoic Ostracoda there is one single form which seems to be an absolutely safe zonal guide, and that is *Cypridea granulosa* (Sow.) the *Cypris fasciculata* of Edward Forbes. It has never been found anywhere but in beds of Middle Purbeck Age, and there it occurs abundantly. Those forms with which it is associated pass up or down into the other beds; more extended collecting, however, may possibly show this species to be more generally distributed.

The Cainozoic Ostracoda, partly treated by Jones in 1850 (*Annals*, ser. 2, vol. vi.), were monographed by him in 1857 (*Palæont. Soc.*), and a revision of this work by Jones and Sherborn appeared in 1889 in the same publication. There are, of course, various separate papers concerning the group, but the information was all collected together in the supplemental monograph of 1889, and references were given to previous literature. The Pleistocene forms received adequate recognition from Dr. G. S. Brady, Mr. Crosskey, and Mr. David Robertson in 1874 (*Palæont. Soc.*); and the recent world-wide species have been described by the first of these authors in the Challenger *Reports*, the *Transactions* of the Zoological Society, and elsewhere; and those of all Western Europe by G. S. Brady and A. M. Norman in 1896 (*Sci. Trans. R. Dublin Soc.*).

Enough has now been said to give a clue to the literature of the

subject and to show the necessity for further work. The valves of Ostracoda are so common in certain beds as to rank among the rock-formers, and they present usually a delicacy and intricacy of ornament and hinge structure which admits of their easy differentiation. The smooth forms present many difficulties; but, with care, specimens can be found which show the hingements, and thus permit determination of the genus, though the determination of the species is much more difficult. What is really wanted is careful collecting from some well-known section, or series of sections, of any geological formation, and this would be of greater importance than the casual description of various species taken at random from many localities, though the latter is, of course, a very necessary work.

C. DAVIES SHERBORN.

540, King's Road, Chelsea.

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V.

Human Evolution.

I.—According to Mr. H. G. Wells.

MR. WELLS' contribution to this discussion in the *Fortnightly Review* for October, 1896, though interesting, appears to me more ingenious than convincing; and his conclusion that "man is still, mentally, morally, and physically, what he was during the later Palæolithic Period," may be attacked on both *a priori* and *a posteriori* grounds.

Mr. Wells' contrast of slowly-breeding mankind with quickly-breeding rabbitkind is undeniable so far as it goes: but does it go very far? His argument depends on the assumption that the plasticity—the tendency to variation—of mankind is no greater than that of rabbits; whereas, if the plasticity of man exceed that of the rabbit more than the fertility of rabbits exceeds that of men, it is obvious that a large part of Mr. Wells' argument vanishes at once. Now, I do not intend to support this proposition; but the onus of proof lies upon Mr. Wells, and unless he can demonstrate the negative, he must abandon part of his argument.

As a matter of fact there is a large amount of evidence of a general character that Mr. Wells will find it very awkward to deal with; for it is one of the fundamental positions of evolutionists that domestication—deprival of natural habitat and natural environment—is one of the most powerful causes of variation. Darwin's writings are full of evidence upon this point; and it is well known to experimenters that the first step in artificial selection is to cultivate or domesticate a plant or animal, since the constitution of the species is thereby so profoundly affected, its normal equilibrium is so much disturbed, that marked varieties may be expected to arise; and until varieties arise, selection, of course, cannot enter upon the scene. Now, seeing that no animal has been so long or so completely removed from natural conditions as man, we should *a priori* expect to find this species more variable, and more unstable in constitution, than any other.

Another potent cause of variation is cross-breeding. To this also no animal has been so long and so thoroughly subjected as man—a fact that renders Mr. Wells' case even weaker. But both the *a priori*

difficulty, and the *a posteriori* difficulty that the evolution of man physically is proof of his wonderful plasticity, Mr. Wells ignores, while bringing forward an *a priori* argument dependent on the unproved assumption that man's constitution is stable and not prone to great variability.

But, setting aside all the foregoing considerations, is Mr. Wells' argument unassailable? No! for his conclusion that each of us *at birth* is scarcely distinguishable, mentally and morally, from Palæolithic Man, necessarily implies, either:—(1) That we are at least equally indistinguishable from one another, and that all the huge differences between modern man and man (of the same race) are attributable to differences in training and nurture; or (2) that there were differences between new-born Palæolithic babies, quite as great, and—with some allowance for comparatively recent psychical selection—proportionately as frequent, as those obtaining to-day.

The mere acceptance of the former alternative as the corollary of Mr. Wells' argument disproves his thesis; for that corollary is contradicted by every man's daily experience. In the moral sphere, everybody recognises that some children are born naturally good, and, though with a minimum of moral training, become the pioneers of moral evolution; whilst others are born naturally vicious, and, in spite of the most careful training, inevitably go to the bad. Even in the same family, and under practically identical conditions of training, we may find both black sheep and white. Mr. Wells' argument from the Wolf-Boys is futile, since there is no proof that a boy with a strong inborn moral bent would sink to the same level as the recorded Wolf-Boys.

In the intellectual ('mental') sphere, to accept this corollary would land us in results so obviously absurd that merely to mention them is sufficient; for it has become a truism that genius is inborn, and that no training, or want of training, can eradicate the inborn differences between a genius and a dunce.

The acceptance of the former alternative being thus fatal to Mr. Wells' argument, let us see what follows from the second. Here I am willing to concede him something in the intellectual sphere; for it has long been my conviction that some intellectual geniuses at any rate must have appeared from time to time among primitive races, and that this 'great-man-theory' may explain such discoveries as the boomerang and the use of fire. But from this admission to the assumption set forth above is too long a stride to be taken without a good deal more evidence than we yet possess. Of course, Mr. Wells cannot afford to admit that the mental differences between Palæolithic babies were similar in kind, but different in degree, to those between modern babies; for that would be to invoke the very potency of selection which his article was written to disprove.

Moreover, there are still two very awkward facts for Mr. Wells to reckon with. First, is it not a fact that individuality is less marked

the lower we descend in the scale of civilisation, and that savages are far more innately alike in every respect than are civilized men; just as there certainly seems to us to be far more inborn individuality, and far less dead-level, among dogs than among sheep and rabbits? Secondly, the very conclusion that Mr. Wells controverts, is a necessary deduction from Spencer's definition of evolution; for, in the mental sphere, increased heterogeneity means increased individuality and decreased 'all-alike-ness.' Mr. Wells will therefore find it necessary to disprove a good deal of "First Principles," in order to clear the way for his own argument.

In the moral sphere, the case is a good deal worse for Mr. Wells. All the same difficulties must be met; and in addition there are several others. First, we have no such presumptive evidence for moral geniuses among primitive man as for intellectual geniuses, so that Mr. Wells must make even larger assumptions. Secondly, a good many, who might concede Mr. Wells his primitive intellectual geniuses, probably would agree with me in thinking it far more unlikely, in view of what we know of sociology and psychology, that there were such marked moral geniuses. Lastly, there is a very damaging fact that Mr. Wells has probably entirely overlooked, one that is, perhaps, fatal to his whole argument. Morality, that is subjective morality (the emotional impulse), is entirely a factor of the emotions; and the emotions are simply the psychical side of, broadly speaking, visceral changes. How potent a factor in morality is this visceral change, may be realised if we reflect upon the profound emotional differences between men and women. Let Mr. Wells carefully study Havelock Ellis' "Man and Woman," and he will see how strong is the case against him, and how vast a difference in moral impulse may reasonably be attributed to a comparatively slight alteration in man's visceral physique.

But even did we waive every objection, and grant Mr. Wells the second alternative corollary from his argument, how would this benefit him? His whole drift is that nurture does everything, and Nature almost nothing, for man's advance. How then does it help him to assume that moral and intellectual geniuses were as characteristic "sports" among our Palæolithic ancestors as among ourselves? For, were this the fact, there were no need of Mr. Wells' alarmist article; and he would be only endorsing the oft repeated warning that we ought carefully to breed from our most gifted stocks, and strive to repress the great increase of the worthless.

Several of the arguments here only outlined are set forth more fully in chapter vii. of my "Towards Utopia" and in the *Free Review* for February, 1895. To Mr. Francis Galton's works on heredity I attach so great value that I venture strongly to commend their study to Mr. Wells and those interested in his argument.

In conclusion, I will only add that Mr. Wells would perhaps make out a far stronger case were he to invert his argument; and,

instead of maintaining that, because we do not differ at birth from Palæolithic babies, therefore we are devoted to savagery unless saved by nurture, to urge that the progress already made proves our descent from a gifted stock innately destined to great things. He himself, curiously enough, admits racial differences among mankind, and he can find a mass of arguments in Galton's writings, in the results of recent archæological research, and, among other treatises, in Weismann's "Essay on the Musical Sense." I should be interested to see what Mr. Wells' ingenuity could effect with this inverted argument. After all, what we primarily need is ample discussion, from every point of view, of our anthropological data.

F. H. Perry Coste.

13, Fernshaw Road, Chelsea.

II.—According to Mr. G. Archdall Reid.[1]

To choose a title that shall adequately cover, and no more than cover, the subject of which an author treats is certainly a difficult task. Mr. Reid has failed to perform it. His volume entitled "The Present Evolution of Man" contains a first portion, dealing with Organic Evolution, to which the title gives no clue, while only in the second part of the work is the subject really discussed. On the other hand his title indicates a vast subject of which he considers only a small part. Man's present evolution is dealt with under two sub-headings, Physical and Mental: the former concerned only with man in reference to disease; the latter, with man in reference to indulgence in stimulants and narcotics. This is certainly a very partial treatment of a subject with such possibilities as the present evolution of Man.

Mr. Reid has somewhat mistaken the term "Evolution." In one case he says (p. 7) "the process of evolution has generally been in an upward direction;" but in other cases he uses the term evolution in opposition to "retrogression." The second use is at variance with the first; and it is incorrect. Evolution, the act of unfolding, is strictly applicable to all the phenonema connected with the succession of organic life, as it has unfolded, does, and will unfold itself.

This unfolding has been in the form sometimes of continued progression, sometimes of retrogression after progression, and frequently of progression in certain features combined with retrogression in others. "Evolution," therefore, does not, as Mr. Reid seems to think, imply only progress.

There is yet other misuse of terms, as in such sentences as these (p. 21): "In every species natural selection as a cause of evolution [progression], and atavism as a cause of retrogression, are constantly at

[1] "The Present Evolution of Man." By G. Archdall Reid. Pp. 370. London: Chapman and Hall, 1896. Price 7s. 6d

war. If the one force predominate we have evolution, if the other, retrogression; if the two forces balance one another, the species in which this occurs undergoes for a time neither evolution, nor retrogression." To speak of natural selection as a cause of progression, is certainly a debatable point; but atavism cannot be called a cause of retrogression. It is a phenomenon of retrogression; and it is certainly not a force. Atavism, indeed, is a much misunderstood term. Some writers have called the features of the normal infantile stages of ontogeny, such as monkey-habits of a child, atavic; which is clearly incorrect. Mr. Reid describes atavism as "a failure to recapitulate in the ontogeny the last stages of phylogeny—it is an arrest of development." (p. 47.) He thinks that he is in agreement with Herbert Spencer, who, however, defines it as a recurrence of ancestral traits. But particular facial features appearing in an individual, after having missed several generations, need not necessarily be arrest of development. That would leave a man of our race with a negroid type of nose, such as is common to all young children. It requires a more extended development than obtained in the immediate ancestors to reproduce that highly specialised type of nose, which attracts attention as a characteristic of certain families. Again, another phenomenon often called atavic is the return to ancestral features exhibited in senility, for instance, loss of teeth in man and increasing tendency to a hairy body. For such phenomena there has been proposed the term hypostrophy, which is a truer rendering of the facts as gathered from palæontology.

Rightly enough, Mr. Reid insists that natural and artificial selection are but terms for different exhibitions of one phenomenon, and that man himself is as subject to the laws of evolution as is any other organism. There are naturalists, who so far belie their name, that as soon as they consider man, and what man does, they assume super-natural agencies. Mr. Reid does not make this mistake. Man, indeed, is a most potent influence in modifying the environment of all species, his own included; but though alteration of the environment changes the manner of evolution, it does not stay its action. Man's reason has caused him to cultivate, and so give special forms to, certain species; but in the main his reason has made him the most formidable beast of prey that has ever appeared on the earth. Other animals have taken life to sustain their own. Man does more: he takes it ruthlessly for the gratification of his vanity, for the mere sake of destruction, or from religious zeal. Man's reason has profoundly altered the conditions of environment the world over; and it has been the cause of the greatest misery to the organic world, himself included.

To turn to the first part of Mr. Reid's work, dealing with organic evolution. This is discussed on the assumption of the non-inheritance of acquired characters. "Biologists generally," says the author, "are gradually veering round to this opinion." This may be doubted. Certainly palæontologists favour the Lamarckian teaching, modified by a doctrine of the appearance of characters gradually earlier in each

generation, which is, in fact, a necessity of ontogenetic recapitulation. And it must be confessed that palæontologists have exceptional facilities for observation.

Mr. Reid speaks of "spontaneous variations, caused we know not how" (p. 12). True, we may not know how all variations are caused; but this is no reason for regarding any as spontaneous. It seems safer to suppose that all variations could, if our knowledge were sufficient, be traced to some cause in the environment. For instance, the crumpling of our toes is assumed to result from the wearing of boots. And the character is transmitted. Two of my children, born with straight-toed feet, which would not disgrace a savage, and kept bare-footed for this very observation, showed a crumpling in of the toes before boots were taken to at all.

There are other causes of variation. I have noted in my own children that a short period between one birth and another has unfavourably influenced the offspring; that change of scene by the mother has been favourable; that a vegetarian diet during gestation produced a very small child; that change back to a meat diet produced the largest child of the family; and lastly, visits to the Inventions Exhibition by the mother during the period of gestation resulted in a child which, unlike the others, has shown, since infancy, a remarkable mechanical proclivity. Now such variations might by a casual observer be called "inborn"; they would be classed as "spontaneous variations, arising we know not how."

Mr. Reid does believe in the transmission of acquired characters in the case of unicellular organisms (p. 134), and this, perhaps, explains the whole matter. The germ is a unicellular organism, and therefore it should be modifiable in accordance with its environment. Such environment would be different in the body of a sedentary clerk, and a hard-working agricultural labourer; and on this hypothesis the offspring in these cases would be different.

In the special section (Part II.) of his book, Mr. Reid first deals with man's evolution against disease. He says that man improves in the face of disease, that the more he is tried by it, the more he is able to resist, that races which have never known particular diseases, such as measles, are killed, like flies on the approach of winter, by what is to us a mild complaint. This looks remarkably like inheritance of acquired characters. Mr. Reid says it is due to elimination of the unfit. But that, if it were the sole cause, should leave a race better able to resist disease all round. This is not the fact; it is only the individual that is better equipped against such diseases as he has had particular struggles with. This seems as though he were influenced by each struggle, and as if his disease-resisting powers acquired, by use, greater strength of coping with those diseases of which they had knowledge, while not resisting those in which they could get no training; and these characters are transmitted to offspring.

In dealing with man's evolution against stimulants, the author

takes a similar line of argument: that those races which have the most experience of alcohol are best able to resist it;—he regards this as due to elimination of the unfit. He gives statistics to show that abstainers live longer than non-abstainers; but they prove nothing; they certainly do not show that thereby abstainers become more numerous. Let us take a case: there are women who abstain from childbirth, others who do not abstain. It is certain that the abstainers live the longer, because the risks of childbirth are many, and result in the loss of many lives. Therefore, in course of time, by the elimination of non-abstainers, only abstainers should be left. It is a *reductio ad absurdum*; but it signifies that tables of comparative longevity do not affect the matter, unless it can also be shown that those who live longest produce most children. This does not follow in the least; and again it is highly probable that non-abstainers are more productive than abstainers. We have evidence on this point. "Total abstinence is more practised by the steadiest men in other respects, though it is also true that the fact of abstaining assists in promoting continence." (p. 322). On the other hand, alcohol certainly stimulates the sexual instinct. Agricultural labourers indulge very largely in drink; and at the same time, they have very considerable families.

Anyone who has paid attention to the population question, who knows the early marriage-rate of farm servants, and their large families, has necessarily come to the conclusion that they produce far more than their share of the next generation. This means that the professional classes must be largely recruited from time to time from those whose forbears were farm servants a few generations ago; and we know that such is a fact. We have no data in this country to show what does happen; but Karl Pearson has proved that in Copenhagen the artisan class beats the professional, and that "the population would appear to be ultimately reproducing itself from the artisan class" (NAT. SCI., vol. viii. No. 51, p. 325, May, 1896). In this country the surplus from the farm-labouring class of each generation joins the artisans. Professor Pearson's statement thus confirms our surmise, and it really means that the greater bulk of our population is being reproduced by those classes who indulge the most in alcohol. The temperate middle and professional class is known to marry latest of all, and it is by no means prolific. Such considerations cut the ground away from many of Mr. Reid's arguments concerning man's evolution in respect of alcohol.

It is strange that a work entitled "The Present Evolution of Man" should take no account of the reproduction question. For instance, there is at the present day a sudden decrease in the birth-rate—stated recently for London as 15 per cent. below the decennial average, which itself has been growing less. This implies that the population is less by hundreds of thousands, and it undoubtedly corresponds with the rapidly-spreading knowledge of chemical means for checking fecundation. However beneficial a small family may be

to the individual, it is not to the good of the race; in fact, all evolution shows that the good of the race and of the individual are antagonistic. The less fertile race will inevitably succumb to the more fertile, and this decrease of the birth-rate heralds its disappearance. It is remarkable that a book dealing with the evolution of Man says nothing of a factor more important than any of which it treats.

However, Mr. Reid's work does well to call attention to man's evolution in certain respects. He says it is a book for the general public, and to that public it may be commended. Unfortunately, the taste of the general public is for works on evolution that show bad science and worse argument, well spiced with theology, and Mr. Reid's book is certainly not of that kind: it leaves theology alone. One may dissent from some of his statements and many of his conclusions; still he has a right to the position he takes up, while his arguments deserve and require careful consideration. His attempt to arouse public attention is praiseworthy, and deserves success.

S. S. Buckman.

Charlton Kings, near Cheltenham.

VI.

Science in New Zealand.

A RECENT number of *The Press* of Christchurch, New Zealand, contains an interesting article by Professor Arthur Dendy, on the present position of science in New Zealand. It is very largely a lamentation over the apathy displayed by the members of a new community. "Our best and most promising University Students," says the Professor, "are obliged to leave us in order to find a market for their intellectual wares, and those few scientific men who have taken up their abode in this colony must still look mainly to Europe and America for an audience." The comparative isolation of the few centres of intellectual activity in the islands is also a great hindrance, since there can be no central library or museum readily accessible to all workers, and funds are not forthcoming to establish such at more than one point. The New Zealand Institute, as the representative scientific body of the colony, is thus seriously handicapped in its career of usefulness compared with the similar institutions in the other Australasian colonies. Instead of being one flourishing and influential society, with first-class accommodation for the meetings and for the library in the capital city, the Institute consists of a "Board of Governors," most of whom are nominated by the Government, and a "Manager," with a number of "incorporated," but practically independent, local Societies in Wellington, Auckland, Canterbury, Otago, Westland, Hawke's Bay, Southland, and Nelson.

It is, indeed, "to a certain extent, a Government institution, and as such receives an annual subsidy from Parliament of some £500, which just about pays the expenses of publication of the annual volume. The incorporated Societies secure the advantage of having their proceedings, including such papers as are deemed worthy by the authorities, published and distributed free of cost to themselves, so that the whole of the annual income of each is available for local purposes. That this is a great advantage cannot be doubted; but, at the same time, it is an arrangement which has, at any rate from the point of view of the Incorporated Societies, serious drawbacks, especially with regard to what may be called the Library Question. Out in these colonies, remote from all the great centres of intellectual activity, the scientific investigator is peculiarly dependent upon the literature of his subject for keeping himself abreast of the times, and it is therefore of the greatest importance that the Society to which he belongs should maintain its library in as efficient a state as possible, for in the great majority of cases he is not himself in a position to

spend much money in the purchase of costly books. Most learned societies keep up their libraries very largely by means of the exchange of their own publications for those of similar institutions throughout the world. In this colony all the advantages of such a system of exchange are confined to Wellington, the headquarters of the Institute, and the local societies outside Wellington profit nothing thereby. It is true that a rule provides for the admission of the public to the use of the Museum and Library, subject to bye-laws to be framed by the Board, but this privilege can be of little value to any but residents in Wellington. The difficulty might, perhaps, be to a certain extent obviated by printing and circulating a catalogue of the Library, and making arrangements whereby members of the affiliated societies could have books posted to them."

"If the support given to the local branches of the Institute can be regarded as any indication of the degree of culture in the different centres of population in New Zealand, we may arrive at some interesting results from a comparison of the lists of membership. Thus we find Auckland first with 172 members, Wellington 144, Otago 94, Hawke's Bay 80, Canterbury 70, Westland 62, and Nelson 24, a result which certainly would not be expected by any person conversant with the higher educational institutions in these centres, and especially with the working of the University Colleges. The degree of popularity and success of the University Colleges appears to have no relation whatever to the prosperity (as judged by membership) of the local branch of the Institute. If, however, we make the comparison in a different manner, by observing the amount of original research emanating from each branch, I think the University centres may fairly claim to have done their share."

The most galling thought to an enthusiast like Professor Dendy is that, while he and his colleagues reside in an almost unique country for opportunities of scientific research, they are "almost forced to neglect them for want of encouragement." Meanwhile, "scientific men from Europe and America, often assisted by a grant from some learned society, hail with enthusiasm every chance of visiting the Australasian colonies, where they remain for a few months collecting, and then return home laden with spoil to work out their results at leisure and under the most favourable conditions." At the same time, even Professor Dendy himself admits that circumstances are improving. The Canterbury College, for instance, has lately exhibited noteworthy progress, and the Canterbury Museum is now one of the finest institutions of its kind in Australasia. Nor must we forget Dunedin, which has done so much for the progress of natural science in recent years. We have Professor Dendy's "man in the street" in this country, and no inconsiderable development of that relic of barbarism. Let us hope that culture in other quarters may soon as completely overpower him in the remotest colonies of Australasia as it has done in the older centres of civilisation.

SOME NEW BOOKS.

Worms, Rotifers, and Polyzoa.

The Cambridge Natural History, edited by S. F. Harmer and A. E. Shipley, vol. ii. Flatworms and Mesozoa, by F. W. Gamble; Nemertines, by Miss L. Sheldon; Threadworms and Sagitta, by A. E. Shipley; Rotifers, by Marcus Hartog; Polychæte Worms, by W. B. Benham; Earthworms and Leeches, by F. E. Beddard; Gephyrea and Phoronis, by A. E. Shipley; Polyzoa, by S. F. Harmer. 8vo. Pp. xii, 560, with numerous illustrations. London: Macmillan and Co., Ltd., 1896. Price 17s. nett.

This second volume of the Cambridge Natural History is certain to prove a most welcome addition to English zoological literature. It deals with a series of animal groups, all deeply interesting to the specialist in morphology; some important from their economic relations to other living things, others in their life-histories rivalling the marvels of fairy-tales. And the style in which they are here treated is also interesting: history and the early observations of the older writers lend their charm; accounts of habits and mode of occurrence, of life, in a word, from the cradle to the grave, are given in ample detail, accompanied by full references to modern and current literature. The whole is admirably illustrated: the figures, as may be seen from those kindly lent us by the publishers, well chosen, well executed, clear and of excellent size. British species are freely alluded to;

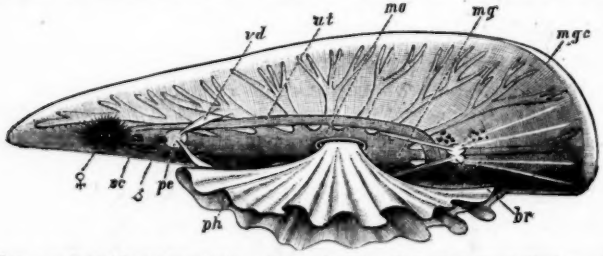

Fig. 1.—*Leptoplana tremellaris*. Example of a Polyclad Turbellarian. Three-quarter ventral view, ×6. *ph*, pharynx protruded from *mo*, mouth; *br*, brain; *mg*, stomach; *mgc*, marginal groove; *pe*, penis; *sc*, sucker; *ut*, uterus; *vd*, vas deferens; ♀ female genital aperture; ♂ male ditto.

indeed the account of the Polyzoa is avowedly based solely upon them. The work will appeal to the student working for examination, but it ought also to stimulate in the amateur naturalist an intelligent interest in animals too much neglected and ignored by him, usually because short yet comprehensive and not too technical accounts of their structure and classification are unavailable. The want is now charmingly supplied. Nowhere, save in monographs, will the lover of pond-life find a better description of the Rotifera, or the wanderer by the sea-shore of the Polyzoa. Indeed, help in relation

to these sprightly beings is extended to include methods of preservation. The index, to crown all, is a thoroughly good and full one.

The volume opens with the flatworms or Platyhelminthes,—a phylum with three branches, Turbellaria, Trematoda, and Cestoda, organised on a similar and very distinct plan. The Turbellaria are dwelt on at some length as occupying "the lowest position in the whole group of worms," and "most closely allied to that great extinct

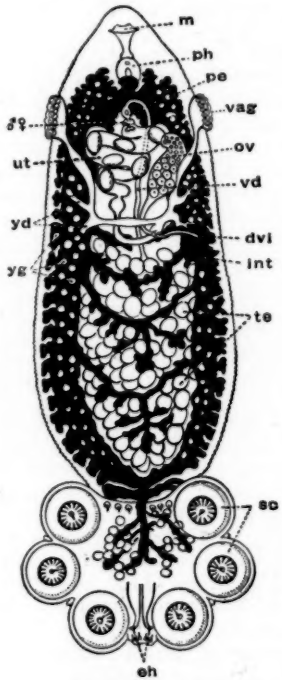

FIG. 2.—*Polystomum integerrimum*, a Trematode from a frog's bladder. Ventral view, × 8. *dvi*, ductus vitello-intestinalis; *eh*, hooks of sucking disc; *int*, intestine (all black); *m*, mouth; *ov*, ovary; *pe*, penis; *ph*, pharynx; *sc*, suckers; *te*, testes; *ut*, uterus with eggs; *vag*, left vagina; *vd*, vas deferens; *yd*, yolk-duct; *yg*, yolk-glands; ♀ ♂ common genital aperture.

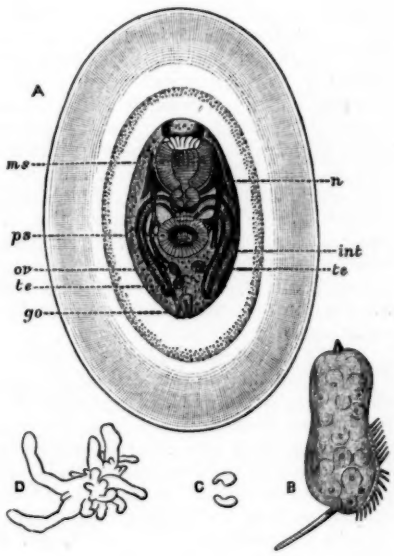

FIG. 3.—Life history of *Distomum macrostomum*, a Trematode. A, tailless cercaria from a *Leucochloridium*, × 40; B, larva as hatched from the egg, × 125; C, metamorphosed larva (sporocyst) that has pierced intestinal wall of *Succinea*; D, actively growing sporocyst, or *Leucochloridium*; *go*, genital aperture; *int*, intestine; *ms*, mouth sucker; *n*, nervous system; *ov*, ovary; *ps*, ventral sucker; *te*, testis.

group, from which they, the Nemertinea, Rotifera, and even the Annelids" appear to have been "derived." A full description of the Polyclad, *Leptoplana tremellaris* (fig. 1), with good figures, is given; and a no less good figure of the anatomy of the Triclad, *Planaria lactea*. The division of the Trematoda into Mono- and Di-genea is retained, though with a protest (p. 63). There is an excellent account of the Turbellarian-like Monogenetic family, the Temnocephalidæ, which has no British representative. Mr. Gamble believes in the existence,

repeatedly denied, of a ductus vitello-intestinalis uniting uterus and intestine in *Polystomum* (fig. 2), and in its homology with the Laurer-Stieda canal of Digenea. The life-history of the Digenea is exemplified by that of *Distomum macrostomum* (fig. 3) and its sporocyst *Leucochloridium paradoxum*, which, by the way, has been found in two localities in Oxfordshire. The selection is unfortunate, for the sporocyst is one of the two most peculiar known, utterly unlike the ordinary form; and the cercaria is not typical. The anatomy and ordinary form of the sporocyst and cercaria are not described, and the redia is only cursorily mentioned. Yet sporocyst and redia are dimorphic forms of fluke, with a by no means simple structure. A useful table of life-histories is given, and a similar one for certain Cestoda, as well as a second table for the identification of the tape-worms inhabiting man and the domestic animals. Mr. Gamble evidently leans to the view that there is no alternation of generations in the life-history of the majority of tape-worms. Scolex and proglottides are parts of one individual. To the facts adduced it might be added that in *Ligula* and *Schistocephalus* it has been said that the vitellaria, their ductules, and the array of testes are continuous throughout the body; so too with the germaria of *Ligula*. In *Tænia echinococcus* the vesicular embryo multiplies *quâ* vesicle, a number of scolices arising from the secondary, etc., vesicles; though *T. cænurus* rarely multiplies as a vesicle but always develops many scolices, the two species are regarded as "instances of an alternation of asexual generations in the larval state with a sexual adult state."

The Mesozoa of E. van Beneden are treated as an appendix to Platyhelminthes, as they are by other authorities. It may be conceded there is no proof of the Dicyemidæ and Orthonectidæ being forms of life intermediate between Protozoa and Metazoa: there is equally no proof that they are related to one another or that they are derived from platyhelminthic ancestors, simplified by an extreme degeneration due to parasitism. Before a sporocyst was transformed into a dicyemid, its structure must have been sublimed out of recognition.

In her account of Nemertinea, Miss Sheldon has been able to use the results of Bürger's recent monograph only in the modified classification and the account of the excretory organs with their terminal and platyhelminthic flame-cells. Nothing is said as to the nature of the blood-vessels, whether cœlomic or not. The land and fresh-water forms are enumerated, and in part described, together with the parasitic (?) leech-like *Malacobdella*. Detailed reference is made to the views of those who find features of alliance to the Chordata.

The old name Nemathelminthes unites the Nematoda (thread-worms), Nematomorpha (*Gordius*, *Nectonema*), and Acanthocephala. It is an echo of Platyhelminthes, and its use, therefore, is to be deprecated. Mr. Shipley does not base the collocation on phylogenetic grounds, but on convenience and a greater similarity between the three groups *inter se* as contrasted with other groups of animals. Camerano's argument as to the similarity of the "chief details in the fertilisation aud development of the egg" of *Gordius*, with the similar processes in Nematoda, does not outweigh considerations of anatomy; and if Kaiser's description of the nephridial organs of *Gigantorhynchus gigas* be correct, the Acanthocephala must certainly be severed from the two other groups. An unfortunate error has crept into the account of *Rhabdonema nigrovenosum*. The parasitic generation in the frog's lung is a female in structure, but, physiologically, a protandric

hermaphrodite: it is not the case that no females occur in the alternate generation (p. 151), or that there is parthenogenesis (p. 161).

The Rotifera have always proved attractive to the non-professional naturalist. There are, however, certain difficulties in their anatomy, and these are clearly elucidated in Professor Hartog's account of the class. The varieties of trochal disc, the structure of the mastax, reproduction, and habits are fully dealt with. The classification is modified somewhat from Hudson and Gosse's, and the Professor broaches a new and interesting idea as to the zoological affinities of the class, bringing it "into close relationship with lower Platyhelminthes," especially the Pilidium larva; he rejects any affinity to Crustacea.

The four orders, Archiannelida, Polychæta, Myzostomaria, and Oligochæta, are ranged by Dr. Benham under the term Chætopoda: he deals with the first three. The remarkable genus *Dinophilus* is included, apparently with some reluctance, in the order first-named; *Saccocirrus* is omitted, and the interesting *Histriobdella homari* relegated to a foot-note. In the account of *Polygordius* no mention is made of the development of the excretory system, neither is its trochosphere figured; indeed, this important larval form is nowhere treated adequately in this volume.

Nereis pelagica (fig. 4) is described as a type of the Polychæta, but the description is too much interrupted by facts of general anatomy.

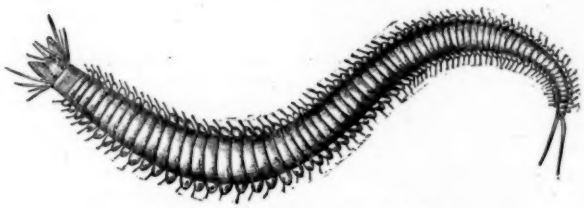

FIG. 4.—*Nereis pelagica*, nat. size.

The classification of the order adopted is the one proposed in 1894 by Dr. Benham; but any difficulties owing to its recent character are minimised as far as possible by the full account to which chapter xii. is devoted. Certain errors (*inter alia*) in general anatomy may be noted. The septa of the body are not so complete as to isolate the compartments from one another, and the parapodia are essentially hollow organs, facts not explicitly stated; prostomial tentacles are probably not restricted solely to the sub-order Nereidiformia, *e.g.*, according to Mayer, the tentacles of *Spio* may be prostomial; the modified chætæ of *Capitella* are confined to the male, and modified anterior chætæ occur in other Polychæta; all polycirrids are devoid of a vascular system, and, in *Capitella*, some, not all, of the cœlomic corpuscles are red; the siphon is found in some Eunicidæ as well as in *Capitelliformia*; the longitudinal nephridial duct of some Terebellidæ, *e.g.*, the common *Lanice conchilega*, is omitted; *Capitella* has one pair of gonoducts, other genera of the family more than one pair; *Haplobranchus* is not hermaphrodite. A whole chapter (chapter xi.) is devoted to a *résumé* of the natural history of the order.

Turning to the Oligochæta, the order is for once treated with complete justice, and by its monographer, Mr. Beddard; the Hirudinea are brought into intimate relation with it, the differences between the two being, as he says, neither so "numerous" nor so

"important" as they are usually held to be. It may be noted that the stomatogastric nervous system of Oligochæta is often connected to the œsophageal commissures, and the chambers of the body are not completely isolated *inter se* as might be inferred from the language of p. 355. The spermatophore of the order is mentioned under Hirudinea (p. 402). Goehlich's observation on the nephridia, reproductive habits, and cocoon of the earthworm should have found a place. It is a pity Mr. Beddard did not figure *Chætogaster*, *Nais*, or *Tubifex* as common British types, or give details as to the fission of aquatic forms. The circulatory system, cœlomic spaces, and genitalia of Hirudinea are clearly explained. There is a brief reference to the

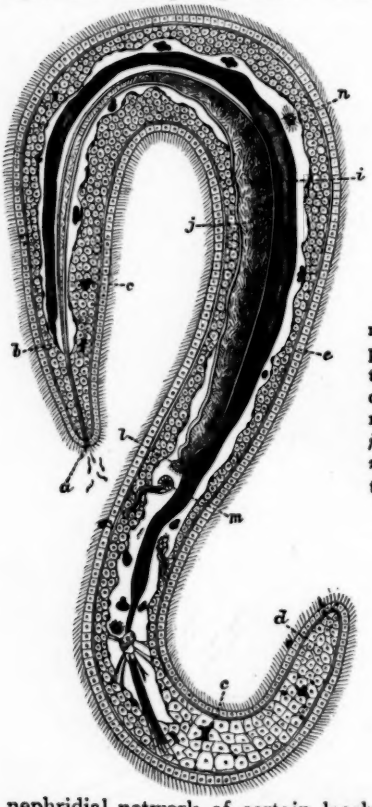

FIG. 5.—*Bonellia viridis*, a Gephyrean, adult male × 133. *a*, generative pore with spermatozoa issuing; *b*, anterior end of intestine (*i*); *c*, wandering cells containing chlorophyll; *d*, parenchymatous connective tissue; *e*, epidermis; *j*, vas deferens opening internally at *l*; *m*, "left nephridium" (Shipley, but see text); *n*, spermatozoa in body-cavity.

nephridial network of certain leeches. Mr. Beddard probably considers it, as in analogous instances among Oligochæta, to be a development from simpler conditions.

Sipunculus nudus is described as a type of the Gephyrea, and Mr. Shipley has given a welcome key to the many genera of the Sipunculoidea. An error should be noted in the description of the figure of the male *Bonellia* (fig. 5): the letter *m* refers to what is really the homologue of the left anal vesicle of the female; the homologue on the right side is also shown. The remarks on the affinities of the Gephyrea should be carefully studied. The same advice applies equally to the interesting genus *Phoronis*, of which an excellent description is given.

The Polyzoa conclude the volume. Mr. Harmer bases his account on British species solely, regarding them as amply representative, a statement which hardly applies to the Cyclostomata. The controversy as to the name of the class is alluded to; many English writers are certainly inclining to Ehrenberg's name Bryozoa. The Entoprocta are rightly retained as one of the two chief divisions of the class. The brown body of the Ectoprocta, the derivation of the avicularium and vibraculum, and the morphology of the larva are explained; and the view that the fresh-water Phylactolæmata are not primitive forms, as usually stated, is put convincingly. The references to the palæontology of the class are scanty: *e.g.*, Ulrich's work on American Palæozoic forms is passed over; Walford's results are accepted as trustworthy, whereas he seems to have mistaken accidental scratches and the like for structural features; existing families of Cyclostomata commence *before* the Jurassic period, even in British Silurian and American Carboniferous strata. There are two useful tables for the identification of British fresh-water and marine species respectively. As to the latter, the student should bear in mind the caution given on pp. 515–16 concerning the form of the colony, especially as to encrusting and erect forms. Note that *Epistomia*, and not *Notamia*, is the correct generic name for the species *bursaria* (p. 526).

Consideration of the whole volume suggests a few general remarks. There is a certain vagueness, sure to puzzle the non-professional student, in the use of the terms *body-cavity* and *cœlom*, and the connections in which they occasionally appear. Not that it is confined to this work; still, it could easily be avoided. The absence of brief definitions to the principal groups, such as are given in the companion volume on Arthropoda, is a misfortune. They certainly conduce to clearness of ideas, and are indispensable in certain stages of knowledge. Classificatory terms are also used in a somewhat haphazard fashion. For instance, the Platyhelminthes constitute a *phylum* with three *branches*, whilst the Nemathelminthes are not designated, and are divided into three *sub-orders*; the Rotifera and Polyzoa are *classes*; the Chætopoda are not designated, and are divided into four *orders*, one of which is the Oligochæta (p. 241); but the Oligochæta on p. 347 are termed a division of the *order* Chætopoda, whilst the Hirudinea, apparently co-ordinate with Oligochæta, are divided into two *sub-orders*. Such things ought not to be. Finally, it is remarkable how sparing is the use of the word "worm," whilst the term Vermes—the term under which the whole contents of the volume were at one time classified—is mentioned, and with a touch of scorn, but once. A prefatory introduction would have been a good thing: it might have explained many essential points and removed difficulties.

Seasonal Dimorphism.

Neue Versuche Zum Saison-Dimorphismus der Schmetterlinge. Von Dr. August Weismann. Abdruck aus den *Zoologischen Jahrbüchern, Abth. f. Syst.* Bd. viii. Jena: Fischer, 1895.

New Experiments on the Seasonal Dimorphism of Lepidoptera. By Dr. August Weismann. (Translated from the German by W. E. Nicholson, F.E.S.) Reprinted from *The Entomologist*, Jan.–Aug., 1896.

Dr. Weismann's essay should be another reproof to those ill-informed persons who cry out on its author as a pure speculator. The misconception is natural; for his theories have reached everyone, even politicians, while the exact laborious work that has accompanied

each theoretical advance has been of a character intelligible only to specialists. The essay now before us gives the results of a series of experiments conducted at intervals, since 1875, upon the conditions determining the seasonal and climatic variations of butterflies. The earliest results were translated into English by Professor Meldola, and appeared among the "Studies in the Theory of Descent" in 1882; the present essay has been well translated by Mr. W. E. Nicholson. The existence of these translations is particularly fortunate, as by them many English entomologists may be stimulated to assist in the important work of collecting additional experimental observations on these curious occurrences. Much good work of this kind is well within the opportunities of the ordinary or field collector, and would afford him nearly as much pleasure as the capture of rare duplicates for exchanges.

The general nature of the circumstances under investigation is as follows: many lepidoptera exhibit, apart from sexual dimorphism, a dimorphism related to climate or to seasons. Some European forms, for instance, have Polar varieties notably different in colour and markings. Some temperate forms have southern, semitropical varieties equally different. Many have two broods; a spring brood resulting from eggs or pupæ that have hibernated, and a different summer or autumn brood. The spring broods tend to resemble northern forms; the summer or autumn broods resemble southern forms. In Lamarckian days most of these differences were set down naïvely to the direct effect of heat or of cold. Detailed experiments show that the explanation is not so fortunately simple; and Professor Weismann devotes his pages to describing his experiments, correlating them with those of others, and discussing their interpretation from the point of view of bionomics and heredity.

In 1875 he assumed that it was to a certain extent obvious that seasonal dimorphism was the direct result of climatic influences—chiefly of heat and cold. He suggested even then the possibility that the seasonal changes were adaptive, like the seasonal changes of Arctic mammals, with the difference that the changes did not occur in one and the same individual, but alternately in individuals of different generations. However, the upper side of butterflies, which is not usually adaptive, varies as much as the adaptive under side in the two generations: moreover, by artificial subjection to a higher or lower temperature, the stamp of the winter form may be impressed on the summer brood, or *vice versâ*. It seemed plain that the measure of heat acting during the pupal period directly formed the species in one way or in the other.

Now, however, he is inclined to the view that there is *adaptive* seasonal dimorphism in addition to *direct* seasonal dimorphism. There are cases known, for instance, in which one seasonal form differs from another, not merely in colouring, but in the presence of complicated details of marking and colouring, such as ocelli; and it cannot be thought that heat or cold, drought or humidity, are the direct causes of these. Professor Weismann thinks that in the embryo there are present the germinal constituents for both forms: the seasonal or artificial change acts as the stimulus to liberate and bring into activity one or the other set. In any particular case it may be that both direct and adaptive dimorphism is present. Very many carefully devised experiments, and the fullest observation of the different forms, alive in their natural surroundings, are required for the discrimination. Professor Weismann discusses his own experiments and those of others so clearly and fully that anyone, with the taste and opportunity,

who reads the pamphlet is like to busy himself at once with some contributions of his own to the problem. We congratulate the conductors of *The Entomologist* on placing before their readers so stimulating a piece of work.

The Common Drone-Fly.

The Natural History of *Eristalis Tenax* or The Drone-Fly. By George Bowdler Buckton. 8vo. Pp. iv., 91. With illustrations, 9 pls. London: Macmillan & Co., 1895. Price 8s. nett.

The drone-fly is among the largest of British Diptera, and fulfils one of the first requirements in the case of an animal selected as a type for scientific study, in that it is exceedingly common. The larva of the insect is of remarkable structure, and the fly itself is peculiarly interesting, especially on account of its deceptive resemblance to the hive-bee. It will therefore be understood that in the hands of a competent writer, the drone-fly might be made to render a real service, not only to dipterology, but to comparative entomology besides. Alas for our fond hopes! The "Natural History of *Eristalis tenax*" has yet to be written.

To deal satisfactorily with the natural history of even a single insect within the limits of some ninety pages of large type demands much careful study, not only of the insect itself, but also of other forms belonging to the same order, not to speak of an intimate acquaintance with the work of previous writers. We are not surprised when Mr. Buckton tells us that his "special attention to *Eristalis*" dates from "the summers of 1893 and 1894." The whole book, including the illustrations, bears unmistakable evidence of hasty work, lack of thoroughness, and want of grip, while disjointed remarks and hazy speculations, couched in high-flown phraseology, are too often made to do duty for a concise and suggestive statement of facts.

The pages devoted to Introduction and Classification contain so many errors that we can only allude to a few of the more glaring. On page 4 and elsewhere, the author perpetuates the time-honoured myth that the larvæ of *Volucella* prey upon those of wasps and hive-bees. The larvæ of *Volucella bombylans* and of other species of the same genus are very common in the nests of *Vespa germanica* and in those of humble bees, certain species of which *V. bombylans* closely resembles; but it is now known that they act as scavengers, feeding merely upon the refuse of the cells, and not upon the progeny of their hosts. (See Natural Science, vol. ii., p. 54). At the foot of page 6 (where, by the way, the Oestridæ are twice called *A*stridæ), we have a table purporting to show the systematic position of *Eristalis* according to Walker, in which, while the Nycteribidæ are allowed to represent the Eproboscidea, the much better known family Hippoboscidæ apparently belongs neither to this section nor to that which is still blest with a proboscis. Why are Prof. Brauer's researches upon the classification of Diptera, and Williston's "Synopsis of North American Syrphidæ" —the most important revision yet attempted of the family to which *Eristalis* belongs—completely ignored?

On the following page we are told that in predaceous Diptera the *labium* "operates like a piercer," and a little later that "the imagos of Diptera possess no mandibles." Statements of this sort simply take one's breath away, and in quoting them it is difficult to refrain from copious notes of exclamation. "*Eristalis arbustorum*, Linn." is quoted quite correctly on page 8 as the name of a distinct species, but on the following page it appears as a synonym of *Eristalis tenax*.

Mr. Buckton's description of the imago of *E. tenax* would certainly not enable anyone to recognise the fly, and he makes no mention of the two vertical stripes of hair on the eyes, which alone are sufficient to distinguish the species from any other member of the genus. In the description of *Eristalis arbustorum* on page 15, we are told that the eyes are "approximate at the vertex," although this species is no exception to the rule that in the majority of what we may call ordinary Diptera, the eyes in the female are wide apart.

The chapter on "Life History" is quite the best portion of the book, and we gladly recognise that the author has endeavoured to study the living insect. But again we find serious errors of commission and omission. *Pipunculus*, which belongs to a family nearly allied to the Syrphidæ, is called a *gnat* (p. 19); the author probably intended to write *Chironomus*; the larva of *Eristalis tenax* is said to possess eight pairs of false feet, and that of *E. arbustorum* seven (p. 21), which is exactly the reverse of what we were told seven pages previously. Moreover, considering the title of the book, we think we are justly entitled to complain that while nearly forty pages are devoted to anatomical details, the life-history of the insect (if we exclude four pages on the "Bugonia" myth later on) is dismissed in less than twelve. We fail to find any reference to the protective value, or otherwise, to the insect itself of its remarkable similarity to the hive-bee, and the author does not appear to have made any experiments upon the subject.

With regard to the anatomical portion of the book, the author certainly seems to have plied scalpel and dissecting-needle assiduously, but his observations are too often confused and superficial, he fails to grasp the really important points of his subject, and he manifests throughout a sadly imperfect acquaintance with the work of previous writers. In his account of the internal structure of the eye he makes no reference to Hickson, in dealing with the halteres and their function he omits to allude to Jousset de Bellesme's book upon the subject, and in his remarks on the nervous system he says nothing about Brauer's memoir. Moreover, we are apt to distrust the author's statements as to obscure points of internal anatomy when, with regard to well-known external details with which every entomologist is familiar, we find such flagrant misstatements as:—"The metathorax is . . . . . principally represented by the scutellum." . . . . . "The wings are affixed to the metanotum." The account of the structure of the proboscis, too (pp. 35–37) is quite unintelligible, and the figure on Plate V. merely serves to make confusion worse confounded.

The pages headed "Development of Eristalis" are found on examination not to refer to the ontogeny of the insect at all, but to consist for the most part of vague musings upon the ancestry of the Tracheata. The value of Mr. Buckton's reflections may be deduced from the following sentence, which apparently is intended to convey the author's last word upon the subject:—"Certain considerations would lead us to the belief that aerial came from aquatic forms; but from the incomplete state of our knowledge caused by want of material, generalization at present would appear to be very unsafe."

HORTICULTURE AND EVOLUTION.

THE SURVIVAL OF THE UNLIKE: a collection of evolution essays suggested by the study of domestic plants. By L. H. Bailey. 8vo. Pp. 516. New York: The Macmillan Co; 1896. Price 2 dols.

THESE reprinted essays, written primarily for horticulturists, form a valuable contribution to the study of evolution. The underlying

motive of the collection is the emphasis placed upon unlikenesses and their survival because they are unlike. Moreover, it is not variation that requires explanation, but heredity, for "normally, or originally, unlike produces unlike."

The first conclusion of those who studied wild plants was that species were fixed entities, originally created as we now see them, and that, if they varied, it was within narrow limits. With cultivated varieties the systematist had nothing to do. *Brassica oleracea* might have innumerable varieties under cultivation, but in nature it was apparently a fixed type. Mr. Bailey, however, refuses to keep the botanist outside the garden-fence, and would consider the forms of *B. oleracea*—cabbage, cole-wort, cauliflower, and kohl-rabbi—as four sub-species. But, though these and other garden-types, such as parsnips and radishes, maintain persistently the characters impressed on them by the horticulturist, yet the many races of each are in a constant state of flux. Mr. Bailey, as a horticulturist, is so impressed by this universal change, that he considers a new character to be useful to a species simply because it makes it unlike its kin. But if it be a just inference that the varieties of the cabbage survive because they are "unlike," it is equally logical to conclude that the wild *B. oleracea* survives because it is "like." A study of plants under both conditions suggests that a constant environment cannot induce the plant to change, but that a plant which meets with "changed conditions of life" (Darwin) may vary to any extent in response to them. The differences, as Mr. Bailey puts it, "find the places of least conflict, and persist because they thrive best. . . . There are, therefore, as many species as there are unlike conditions in physical and environmental nature." This is only true if "species" stands for any degree of variation. "To Nature, perfect adaptation is the end; she knows nothing, *per se*, as species or as fixed types. Species were created by John Ray, not by the Lord; they were named by Linnæus, not by Adam."

The thesis of original variation is based on such individual variations of plants as were discussed in NATURAL SCIENCE, vol. vi., p. 385. "Inasmuch as no two individual organisms ever are or ever have been exactly alike, so far as we can determine, it seems to me to be the logical necessity to assume that like never did and never can produce like." "It is a more violent assumption to suppose that the first unspecialised plasma should exactly reproduce all its minor features than to suppose that it had no distinct hereditary power, and therefore, by the very nature of its constitution, could not exactly reproduce itself." This view is supported by the time it takes a breeder to fix any new character; five years is perhaps the average period for plants. Too much stress, however, must not be laid on individual variations, which really are nothing more than results of inexact growth and unequal distribution of nutriment, for no organism can be "absolute" in nature; and they do not give rise to varieties so long as the environment is unchanged. If a "species" or "form" be regarded irrespective of these individual variations, then it is true that "like produces like." Consider it as Mr. Bailey does, and it is equally true that "unlike produces unlike."

The facts of horticulture lead Mr. Bailey to summarise this part of his book thus:—"Unlikenesses in plants are (1) the expressions of the ever-changing environmental conditions in which plants grow, and of the incidental stimuli to which they are exposed; (2) the result of the force of mere growth; (3) the outcome of sexual mixing. They survive because they are unlike, and thereby enter fields of least competition."

The third essay, on "The Philosophy of Bud-variation," has already been noticed in NATURAL SCIENCE (vol. vii., p. 103). Briefly put, it maintains that new varieties and species can be bred from buds and cuttings just as well as from seeds, although in nature seeds have the advantage owing to their greater dispersal. These phenomena, according to Mr. Bailey, render Weismann's conception of the germ-plasm, not merely unnecessary, but untrue.

Much of the book is devoted to relating the evolution of various horticultural varieties, or even, as Mr. Bailey would call them, species. The main conclusions are brought together in the essay on "Experimental Evolution amongst Plants," which shows that many garden forms are as distinct from one another and from their ancestors as are the corresponding wild forms, and that they are therefore just as much entitled to be called species, for "form" is the only criterion of specific difference. To the demand for experimental evidence of evolution, our author replies,—"The horticulturist is one of the very few men whose distinct business and profession is evolution. He, of all other men, has the experimental proof that species come and go."

GEORGE HENSLOW.

THOMAS SAY AS PALÆONTOLOGIST.

THE fifth *Bulletin of American Palæontology*, issued on December 7, 1896, by Gilbert D. Harris, Ithaca, N.Y., at a price of $1.77 to non-subscribers, turns out to be, not the original monograph for which Professor Harris offered a prize some months ago, but "A Reprint of the palæontological writings of Thomas Say," a piece of work probably quite as valuable. Say's five careful papers were published from 1819 to 1825 in the first series of *Silliman's Journal* (now the *American Journal of Science*) and in the *Journal* of the Academy of Natural Sciences of Philadelphia. Both publications are now scarce. An appendix to this *Bulletin*, issued in January, 1897, reprints various footnotes contributed by Say to the "Account of an Expedition from Pittsburgh to the Rocky Mountains . . . under the Command of Major Stephen H. Long," Philadelphia, 1823. They describe, and in some cases name, fossils collected by the Expedition. It is to Say that we owe the first description of *Pentremites*, which, by the way, he invariably called *Pentremite*, and the foundation of the order Blastoidea; he also introduced *Caryocrinus* and *Exogyra*. The present reprint follows the originals "word for word, line for line, page for page, and plate for plate"; it seems to be intended as an exact reproduction, and therefore three misprints in the second article, "lamarck," "silecified," and "abrubtly," should have been left, as in the original. Surely it would not be much more expensive to reproduce the original text by photo-engraving, just as the plates have been copied. This would obviate all risk of error.

NEW SERIALS.

From Paris we receive No. 1 (Janvier, 1897) of *Revue Critique de Paléozoologie*, a quarterly published under the direction of Maurice Cossmann, 95 Rue de Maubeuge, with an annual subscription of six francs within the postal union. In this number Mr. Cossmann reviews general works and palæoconchology; J. Lambert, echinoderms; and G. F. Dollfus, Bryozoa, zoophytes, and Foraminifera. It is proposed to analyse the important palæontological works of each year, without respect to nationality, while indices of new names and of strata will be provided to each volume. Short rectifications of

nomenclature from correspondents are asked for, but not original articles. The present notices are like those of the *Neues Jahrbuch für Mineralogie*: good abstracts without too much of the abstractor's personal views. An excellent model to follow.

The first number of the *Journal of School Geography*, a monthly of thirty-two pages, edited by Professor R. E. Dodge, of New York, assisted by Professor W. M. Davis, Dr. C. W. Hayes, Dr. H. B. Kümmell, Dr. F. M. McMurry, and Mr. R. de C. Ward, appeared in January. It is intended for teachers, but will interest yet others. Facts are desired, and wordiness eschewed. It contains articles, notes, and reviews. Geographers will do well to send one dollar, being a year's subscription, to 41 North Queen Street, Lancaster, Pa., U.S.A.

Masson et Cie., Paris, announce a new quarterly journal, *Archives d'Anatomie Microscopique*, to be edited by Professor L. F. Henneguy, under the direction of Professors E. G. Balbiani and L. Ranvier.

We have received Nos. 1 and 2 of *Die Umschau*, a weekly journal published by H. Bechhold, Frankfurt-a-M., at a yearly subscription of ten marks, single numbers forty pfennige. It contains short articles dealing with progress and activity in the whole round of pure and applied sciences, literature, and art. Among the contributors are mentioned M. Buchner, K. Busz, W. Flemming, R. H. Francé, O. Hamann, H. H. Hirsch, P. Jensen, R. Michael, A. Nestler, L. Reh, F. Römer, Fr. v. Wagner, and O. Zacharias. The second number contains a paper on cave-fauna by Dr. Hamann, being based on his book "Europäische Höhlenfauna," Jena, 1896.

Bibliography.

In connection with the *Bibliographia universalis*, *Il Policlinico*, an Italian medical review, will, from January of this year, publish regularly a bibliography of current Italian medical literature; the same will be done for Belgium by the *Journal médical*. The movement will also be aided by the *Bibliographia obstetrica-ginecologica italiana* for 1895.

The Geological Society of London has issued Part I. (A–La) of a "General Index to the first fifty volumes of its *Quarterly Journal*," compiled by Mr. L. L. Belinfante, the Assistant-Secretary. The conclusion is promised in August. It is arranged both as author and subject index in one alphabet, and the subject-headings provide a wealth of information in themselves. The care lavished on the preparation is shown by the numerous minor details that have been carefully noted and preserved, and whether one is on the hunt for "Bagshots" or obscure scraps of fossil reptiles, one's time will be saved by this valuable index. A slight fault is the omission of the authority for the new specific and generic names.

Mr. W. Rupert Jones' "Geological Literature added to the Geological Society's Library during the year ended December 31st, 1896," improves on the good qualities of previous issues. It seems a pity that this cannot be slightly extended in scope so as to make it a complete record of each year's geological literature. Publications not in the Society's library might be distinguished by some sign, a perpetual reminder to possible donors.

We have received the fifth and concluding part of the "Bibliography of American Economic Entomology" (down to 1888), issued by the U.S. Department of Agriculture. This comprises a list, under authors' names (L—Z), of papers and notes, and its utility is vastly increased by an index of subjects at the end. A large number of the contributions here recorded would not be inserted in any

general zoological bibliography, and their collection in the present work is therefore of special value. The lists have been compiled, with assistance, by Mr. S. Henshaw. We are glad to see that it is intended to issue supplements, bringing the work up to date.

Various changes in editors have come in with this year. *La Nature* will be edited by Mr. H. Parvil in place of Mr. G. Tissandier. Professors J. S. Kingsley and C. O. Whitman leave the editorial board of the *American Naturalist*, the former being succeeded by Mr. F. C. Kenyon, of Philadelphia. The *Botanical Gazette* has blossomed out in a large number of associate editors representing various American and European scientific centres. Professor H. Marshall Ward is the English representative. Professor J. Seth joins the editors of the *Philosophical Review* (Cornell University). Dr. W. Sklarek is joined, as editor of the *Naturwissenschaftliche Rundschau*, by Drs. J. Bernstein, professor of physiology at Halle; W. Ebstein, professor of pathology at Göttingen; A. v. Koenen, professor of palæontology at Göttingen; V. Meyer, professor of chemistry at Heidelberg; and B. Schwalbe, professor of anatomy at Berlin.

SCRAPS FROM SERIALS.

WHILE we in England are talking about Pliocene Man, and while his Burmese contemporary is to be strenuously defended in our pages by Dr. Fritz Noetling, our American cousins are still disputing over the remains of Man supposed to be in glacial drift. The last piece of important evidence has been sent us by Professor E. W. Claypole, who, in the *American Geologist* for November, 1896, describes a grooved greenstone axe of common aboriginal American type, said to have been found by Mr. Elmer E. Masterman in the tough blue clay below glacial gravel, near New London, Huron Co., Ohio. The specimen when found was considerably decomposed, perhaps by the action of sulphurous water. Other specimens have also been found in or about the same beds by Mr. Masterman. In the *American Naturalist* for January, however, Mr. H. C. Mercer points out that the intended conclusion can hardly be accepted until some professed archæologists shall have witnessed the actual finding of a similar implement *in situ*.

In *Knowledge* for February, Dr. W. F. Hume recalls the fact that specimens of diatoms collected by Nansen from the ice-floes between Iceland and Greenland, and found to belong to species hitherto known only from Bering Strait, as well as particles of minerals collected from the same place, and thought by Törnebohm to come from North Siberia, were among the evidences which caused Nansen to imagine the drift of the ice across the Arctic Ocean. The importance of little things has rarely received more striking proof. This number also contains an article by Harry F. Witherby, entitled "'Twixt Land and Sea," with three half-tone process illustrations, described as "engravings from original oil-paintings by 'A Son of the Marshes.'" Now a picture almost identical with one of these has been hung on our own study-walls for fifteen years, under the supposition that it was a copy of a painting by Wolf. The others also seem familiar. Who is the dupe?

Dr. Bashford Dean has put together a very interesting paper on the Public Aquaria of Europe from notes made during the travels of several years, His paper, which appears in *Appleton's Popular Science Monthly* (November, 1896), deals with Naples, Amsterdam, Plymouth, Paris, Berlin, while Brighton in particular is illustrated and generally described. He remarks that Brighton Aquarium is the most typical, if not the largest, of the newer aquaria of Europe. From Dr. Dean's

account we do not gather that any other aquarium than that of Brighton condescends to the Music Hall entertainment combination.

Memorias de la Sociedad Antonio Alzate, vol. ix., nos. 9 and 10, contains papers by Dr. A. Dugès on the foot of monkeys and the skeletons of the bird and the tortoise, and one by Dr. R. E. Cicero on the medical habits and knowledge of animals.

Readers of Dr. Axel Ohlin's paper. "A Zoologist in Tierra del Fuego" (NAT. SCI., Sept., 1896) should be interested in an account of recent explorations in the Patagonian Andes, 41° S. lat., by Dr. Hans Steffen, in the *Scottish Geographical Magazine*, Feb., 1897.

Additional information concerning the huge octopus that we alluded to in our February number, p. 130, is given by Mr. A. E. Verrill in the February number of the *American Journal of Science*. It is a true octopus, with a body 21 feet long, and almost 7 feet wide. A stump of an arm still attached reaches the length of 36 feet, and was 10 inches in diameter where it was broken off distally. The parts cast ashore probably weigh at least six tons, and the total mass, when living, was probably twice that weight. The drawings of it will be published in the *American Naturalist*. Mr. Verrill names it *Octopus giganteus*, but considers that it may be allied to *Cirroteuthis*. To this number Mr. Verrill also contributes an interesting note on nocturnal and diurnal changes in the colours of certain fishes and of the squid (*Loligo*), with notes on their sleeping habits. The nocturnal colours are obviously protective, and the positions assumed by the fish while sleeping are often quaint and unexpected. In another note Mr. Verrill considers that the nocturnal coloration of various animals has been selected on account of its protective value. This number also begins the long-looked-for outline of a natural classification of the Trilobita, by Dr. C. E. Beecher.

In the *Geological Magazine* for February, Dr. J. W. Gregory discusses the fossils on which Dr. H. Hicks recently based his claim that the Morte Slates of North Devon were of Silurian age. He concludes, from examination of the specimens, that they belong not to the Silurian genera, to which the President of the Geological Society assigned them, but to characteristic Devonian genera. Whoever may ultimately prove right, it is clear that such obscure material as has hitherto been the sole reward of Dr. Hicks' laudable perseverance is not enough to warrant a re-arrangement of the stratigraphy.

FURTHER LITERATURE RECEIVED.

Hemiptera-Homoptera of the British Isles, J. Edwards: Reeve. Manual of Flowering Plants and Ferns, J. C. Willis: Cambridge Univ. Press. Handbook to Lepidoptera, W. F. Kirby: Allen. Catalogus Mammalium, E. L. Trouessart: Friedländer. Life-Histories of North American Birds, C. Bendire: Smithsonian Inst. New Catalogue of British Literature: Library Bureau. Beiträge zur Kenntnis der Septalnectarien, J. Schniewind-Thies; Das Botanische Practicum, E. Strasburger; Fortpflanzung der Gewächse, M. Möbius; Kainogenesis, E. Mehnert; Zeit- und Streit-fragen der Biologie, O. Hertwig: Jena, Fischer. Madreporarian Types of Corals, M. M. Ogilvie: Royal Society. La Machoire des Insectes, J. Chatin: Bailliere.

On Mechanical Selection, K. Jordan: *Novitates Zoologicæ*. Cat. Botanical Works, xvi.; Dulau. On Funifuti Atoll, C. Hedley: *Mem. Aust. Mus.* 10th Report Liverpool Mar. Biol. Com., Herdman. Report Trawling Expedition, A. Meek: North Sea Fish Com. 15th Ann. Rep. Geol. Surv. U.S., J. W. Powell. Dog Muzzling, M. Thornhill: Human. League. Report Glasgow meeting, 1896: Mus. Assoc. Sixth Rep. Horniman Mus. Monog. Australian Marsipobranchii, J. D. Ogilby: *Proc. Linn. Soc. N.S.W.* Article "Biologie," H. de Varigny: Dict. Physiol. Aarbog, 1896: Bergen Mus. Genera and Species of N. Amer. Mosses, C. R. Barnes: *Bull. Univ. Wisconsin*. Closing Tow-net, C. H. Townsend: U. S. Fish Comm. Minn. Bot. Studies, Bull. IX., Geol. & Bot. Surv. Minn.

Nature, Jan. 21, 28, Feb. 4 11. Nature Notes, Feb. Science Gossip, Feb. Naturalist, Feb. Westminster Review, Feb. Review of Reviews, Jan., Feb. Knowledge, Feb. Photogram, Feb. Oxford Univ. Jr. Sci. Club Journ., No. 46. Irish Naturalist, Feb. Scott. Geogr. Magazine, Feb. Victorian Naturalist, Dec. Revue Scientifique, Jan. 23, 30, Feb. 6, 13, 20. Feuille des jeunes Naturalistes, Feb. Naturæ Novitates, Jan. (1 & 2). Literary Digest, Jan. 16. 23, 30, Feb. 6. Amer. Journ. Science, Feb. Science, Jan. 15, 22, 29, Feb. 5. Amer. Nat., Jan., Feb. Botanical Gazette, Jan. Illinois Wesleyan Mag., Dec. Scientific American, Jan. 23, 30, Feb. 6. La Naturaleza, viii., No. 1-4. Journ. Essex Tech. Lab., Nov.-Dec. Hobbies, Jan. 30. Osprey, Jan. Magazin Pittoresque, Jan. 15. Humanity, Feb. Bull. Inst. Bibliogr. 1896, 6 bis, and 1897, 1 & 2. Amer. Geol., Feb.

OBITUARY.

HEINRICH GÄTKE.

BORN MAY 19, 1813. DIED JANUARY 1, 1897.

ORNITHOLOGISTS of all lands will hear of the death of Heinrich Gätke, of Heligoland, with much regret; at the same time it is a matter for congratulation that he lived long enough, with unimpaired faculties, to finish his remarkable work, *Die Vögelwarte Helgoland*, published in German at Brunswick in 1891, and an English edition and translation at Edinburgh in 1895. This, the full value and importance of which to students it is difficult to over-estimate, was the result of more than fifty years' experience and study in the best ornithological observatory on the west coast of Europe by one of the most careful and reliable of observers, one, too, whose labours were practically continuous day and night, for during his long life he was rarely absent from the island.

Gätke was borne in a small town of the Mark of Brandenburg, and it was his desire to become a marine painter that first induced him to visit Heligoland, and finally settle there. Subsequently, during the time of the English occupancy, he held an official position under the Governor. It was constant touch with nature when following his profession which first brought him into contact with that marvellous variety of bird-life, which year by year, with almost rhythmical precision, sweeps to and fro over the lone red rock in its grey setting of sea. I first made Mr. Gätke's acquaintance in 1874, on which occasion, and subsequently, I saw his splendid collections of birds and moths, the whole of which had been prepared and set up by himself, and placed in the well-lighted studio adjoining his house on the Oberland. The total number of species of birds observed on Heligoland during his life was 398, a most extraordinary number for so small a locality, representing, as they did, wanderers from many lands. Every yard, nay, nearly every foot, of the beautiful garden near his house was memorable for some rare capture or occurrence.

Gätke completed the German edition of his work on May 19, 1890, his 77th birthday; the English translation, revised and corrected by himself, in 1895; and very shortly after this he was struck down by partial paralysis. He just lived to cross the threshold of 1897, dying on January 1, in his 84th year.

When Gätke first commenced the study of birds, his guide and text-book was the two Naumanns' great work, *Naturgeschichte der Vögel Deutschlands*. Up to 1874 he had held little communication with English ornithologists. Since that time, however, many naturalists, attracted by the wonders of the island, went over; one result was that he was liberally supplied by Professor Newton, Mr. H. E. Dresser, and others, with all our best and most recent publications on birds.

For twenty-two years the writer carried on a regular correspondence. His last letter, conveying thanks for English reviews of his book forwarded, says, "Such a reception of my work proves indeed a great rest of happiness in the evening of my life."

He wrote as perfectly in English as in German, and his many letters, like his book, are full of beautifully expressed thoughts and word-pictures in connection with his favourite study.

JOHN CORDEAUX.

We regret to record the deaths of:—SIR SPENCER WELLS, M.D., surgeon to the Queen's household, and author of several important surgical works, especially on ovariotomy, on February 1, at Antibes near Nice, aged 79; HENRY BOSWELL, an eminent student of mosses, especially those of Great Britain, at Headington, near Oxford (it is hoped that his large collection will be secured by the University, of which he was an honorary M.A.); DR. EDWARD BALLARD, F.R.S., a distinguished investigator of causes of disease, and promoter of sanitary reforms; R. WARNER, horticulturist, aged 82; on October 3, in Manchester, JOSEPH CHAPPELL, entomologist, aged 67; on September 11, in Paris, DR. EMILE MOREAU, ichthyologist, aged 76; in Troyes (France), the mycologist, MAJOR P. BRIARD; G. F. SCHACHT, who made improvements in the application of certain drugs to the treatment of disease, aged 73; DR. H. VON NÖRDLINGER, formerly Professor of Forestry in Tübingen; at Blumenthal, on December 23, CAPTAIN E. DALLMANN, Antarctic explorer, aged 66; on December 28, in Dantzig, the newly-appointed Director of the Bacteriological Institute, DR. T. J. LICKFETT, aged 49; on January 2, at Munich, FRANZ V. BAUR, a well-known forestry botanist and Professor at the University, aged 66; on October 26, at Linz, DR. A. DÜRRNBERGER, specialist on Salices, Rosaceæ, and Hieraceæ, aged 59; on December 5, the botanist, DR. RUDOLPH RAIMANN, Professor of Natural History at Vienna Commercial College, aged 33; on December 16, the Austrian Botanist, J. ULLEPITSCH, aged 68; on January 13, in Vienna, the well-known cave-explorer, F. KRAUS, aged 63; the conchyliologist, DR. A. L BROT, in Geneva, on August 30, aged 75; DR. S. TRINCHESE, Professor of Comparative Anatomy and Embryology at Naples, and author of many biological works; in Verona, the herpetologist and conchyliologist, E. DE BETTA; J. H. THIRY, formerly Professor of Surgical Pathology in Brussels University; on January 9, at Rotterdam, the Director of the Zoological Garden, A. A. VAN BEMMELEN; DR. S. A. B. LUNDGREN, Professor of Geology in the University of Lund; at Copenhagen, on January 12, C. BAHNSON, a noted ethnologist and Inspector of the Prehistoric Department of the National Museum, aged 42; DR. CHARLES HEITZMANN, histologist, of New York; Professor W. H. PANCOAST, a Philadelphia surgeon; W. H. WARD, of Newark, N.J., a horticulturist; E. G. LODEMAN, Instructor in Horticulture in Cornell University, on December, at Mexico.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

MISLED by an official notice from the Smithsonian Institution, we announced that Mr. F. W. True had been placed in charge of the National Museum, Washington. We now learn that Mr. C. D. Walcott, Director of the U. S. Geological Survey, has been appointed Acting Assistant Secretary of the Smithsonian, with duties confined to the charge of the National Museum. He will, however, only undertake these duties temporarily, without relinquishing his present work. The possibilities of the situation are alluded to in our Notes and Comments.

THE following appointments are announced:—Dr. James Ward, well-known as a physiological psychologist, to be Professor of Mental Philosophy and Logic at Cambridge, where also Dr. E. Barclay-Smith is to be Senior Demonstrator, and F. C. Kempson, Junior Demonstrator, in the Department of Anatomy; Francis E. Lloyd, of the Pacific University, Forest Grove, Oregon, to be Professor of Biology in the Teachers' College, New York; Professor B. Hatschek, of Prague, to the chair of Zoology at Vienna University; Dr. Ch. Julin, of Lüttich University, to be Professor of Comparative Anatomy; Dr. J. Rückert to be Professor of Anatomy in Munich University; Dr. C. J. Martin, to be provisionally the successor to Dr. G. B. Halford as Professor of Physiology in Sydney University; Dr. Hans Lenk, to be Professor of Geology at Würzburg University; Mr. W. Gardiner, Lecturer of Botany at Cambridge, to be Bursar at Clare College; Dr. Alexander P. Anderson, to be Professor of Botany at Clemson College, S.C.; Dr. A. Maggiora, to be Professor of Experimental Hygiene in Modena University, and Dr. A. Serafina, to hold the same position at Padua; Dr. C. Wardell Stiles, as special commissioner for the U. S. A., to report on the parasitic diseases of seal life; Mr. J. G. Luehmann, for nearly thirty years principal Assistant to the late Baron von Mueller, to be Curator of the Melbourne Herbarium; Dr. G. v. Istvanffi, formerly Privatdocent at Buda-Pesth University, and Curator of the National Museum, to be on the staff of Klausenburg University; J. J. Forrester, to be Director of the Royal Agricultural Society's Experiment Station in Woburn, England; Dr. K. Müller, of Halle, the well-known bryologist, and whilom editor of *Natur*, Dr. E. Pringsheim, Privatdocent of Physics in Berlin University, Dr. Karl Eckstein, of Eberswald, Dr. Georg Fischer, of Bamberg, and Dr. Ludwig Plate, of Berlin, to receive the title of Professor. G. Dewalque, Professor of Physical Geography and Geology at Lüttich University, having reached the age-limit, is resigning his post.

THE University of Catania proposes to construct a zoological laboratory on the Island of Cyclops, off the coast of Sicily, which has been presented to them by Signor Gravina.

PROFESSOR J. CHANDRA BOSE, lecturing before the Indian Section of the Society of Arts on February 11, complained that the University of Calcutta was not properly equipped for scientific research, nor was its science-teaching properly organised.

THE Government proposes to ask Parliament to provide funds for the frontage of the South Kensington Museum and for the electric light installation in the National Gallery, National Portrait Gallery, and the Natural History Museum.

Mr. H. W. Moffat has presented to the British Museum (Natural History) a skeleton of one of the ancient inhabitants of Zimbabwe, the authenticity of which was

apparent not only from the peculiar formation of the skull, but from the characteristic gold ornaments still encircling its arms.

The *Echo* states that a unique bronze of a dancing bear is exhibited in the Egyptian Department of the British Museum. It is 7 inches in height, and appears to be of Theban workmanship. The bear was not a native of the Nile valley, and this is therefore intended for the Syrian bear, with which the Egyptians may have become acquainted through the campaigns of Thothmes III. and Rameses II.

WE deeply regret to learn that the prospects of improved accommodation for the valuable collections in Brighton Museum are as bad as ever. Even the Public Library seems to be a disgrace to the town, if we may judge from the mournful report of its Committee. One expects better things of "London-on-Sea." Turning to London, we are glad to see that the fine library of its University, known apparently to about 100 people, is to be rendered more accessible to graduates. Steps that will have a similar effect are contemplated by the Geologists' Association, London, which wishes to deposit its books in the Free Library of St. Martin's parish, of course on certain conditions. We hope, for the sake of both parties, that the negotiations will have a successful issue.

WE have already described the arrangement of the mineralogical and geological sections of the Royal Museum, Peel Park, Salford. We need, therefore, only refer to the appearance of an official handbook to these sections, compiled by Herbert Bolton, and illustrated with figures of fossils and with a less appropriate plate of an entrance corridor with antique statuary. This is the first of a series which it is proposed to issue, and costs 2d.

THE Botanical Museum at Weimar, erected and equipped at the expense of Professor C. Haussknecht, was opened on October 18th., on the occasion of the meeting of the Thuringian Botanical Union. This is destined to be the central station of the Union for researches in systematic botany.

JAMES LLOYD, author of the "Flore de l'Ouest de la France," who died at Nantes on May 10, 1896, has left to the town of Angers his fortune and scientific collections, stipulating that the herbarium should be preserved in its present state in a special room, that the library should also form a special library, placed in the same building as the herbarium, and that the Maire of Angers should select from three candidates proposed by the Botanical Society of France, a curator for both herbarium and library, who should receive a salary of at least 3,000 francs.

THE Council of the Royal Botanic Society has offered a site free of cost for the erection of a students' observatory in connection with London University, together with the use of a lecture-room.

AT the annual Meeting of the Geologists' Association of London, held on February 5th., Mr. E. T. Newton, the President, read a paper "on the evidence for the existence of Man in the Tertiary Period," evidence which he regarded as still inconclusive, despite the efforts of Mr. Lewis Abbott, as described in NATURAL SCIENCE.

THE following extract from Sir Joseph Prestwich's will has been communicated by Lady Prestwich to the Geological Society of France;—"I bequeath the sum of £500 to the Geological Society of France, at Paris, in consideration of the courtesy and friendly co-operation which I have always received from the geologists of that country." This comes into effect on the death of Lady Prestwich, and the income will suffice for a prize to be given every three years. Lady Prestwich will be glad of any letters of her husband's for a biography which she is compiling. These should be sent to her at Shoreham, near Sevenoaks, and will be returned as soon as copied.

AT a meeting of the Hull Geological Society on January 7, Mr. T. Sheppard exhibited a skull from the Millhill gravels, near Brough. It differs somewhat from the typical brachycephalic skulls of the Bronze Age in being longer and narrower. The supraciliary ridges are prominent.

AT a meeting of the Norfolk and Norwich Naturalists' Society, on January 26, the following papers were read:—T. Southwell, on Grassi's work on the development of the eel; W. H. Tuck, on aculeate Hymenoptera from Tostock, Suffolk; A. W. Preston, meteorological notes for 1896; and G. H. Harris, on the Yarmouth herring-fishery of 1896, which was notable for a large catch, the total landed at Yarmouth being 19,252 lasts, and at Lowestoft 8,189 lasts. It was remarkable that this vast host should be "told," that is, literally counted, not in ones, but in fours or "warps." The fish were "told" into baskets or "mands" on deck, thirty-three "warps" or a "long tale" (or long tell) hundred go to a "mand." The "mand" was slid on a plank from deck to quay, carried across the road, and emptied into a larger basket, or "swill." Thirty "swills" went to the "last." A "last" was 13,200 herring, or 3,300 "warps." Mr. Harris suggests that the word "mand," now applied to the basket, was originally the word for the heaps or "mounds" of fish. The other words are clearly of Scandinavian origin—"varp" means a netful or draught of fish; "tal" is a number; "läst" is a ton; "swill" is a very old English word for a herring-basket, probably connected with the Swedish "sill," herring. May it not therefore be that "mand" equals the Swedish "mängd," or multitude?

THE tenth annual Report of the Liverpool Marine Biology Committee is as interesting as the reports of previous years, and contains suggestive remarks by Professor W. A. Herdman on the relations of the Marine Biological Station at Port Erin to the general public, especially that of tender years. Experiments in fish-hatching were conducted on a modest scale, but not without success, at the station during the spring of 1896. Eight dredging expeditions were organised during the year, resulting in the capture of a number of new and interesting species; amongst these is a large green Gephyrean worm, probably a new species of *Thalassema*, distinguished by the possession of a remarkable green pigment, not allied to hæmoglobin or chlorophyll, with a single broad absorption band in the red between C and D. One of the greatest needs of the station is a young chemist or physicist who would join the expeditions with the object of reporting upon the sea-water at the various localities, depths, and seasons. Other items of interest in this Report have already appeared in our news pages.

THE programme of the Scarborough Field Naturalists' Society for 1897 shows that this body continues its good work of popularising science, and of utilising the work of its more enthusiastic though perhaps less trained members. On the latter point Mr. D. W. Bevan offered valuable advice in his presidential address on January 14.

WE regret to learn from the *Revue Scientifique* that the Jardin d'Acclimatation, which was founded in 1858 to conduct experiments in concert with the Société d'Acclimatation on the introduction of foreign animals and plants into France, has now separated itself from the Société, and will form for itself a new society.

THE Cape of Good Hope Government intends to investigate the marine fauna of the South African coast. A small biological station will probably be erected on False Bay, and equipped with a steam launch of 150 tons. Specialists who may wish to work up the material obtained are requested to communicate with Dr. J. D. F. Gilchrist, Agricultural Department, Cape Town. The conditions are as follows: Specimens will be forwarded as procured, and, on receipt of manuscript and drawings, each piece of work will be published without delay in a uniform style, so as to form ultimately a complete record of the Cape marine fauna. Authors'

copies will be forwarded as soon as published, and a certain circulation will be guaranteed. No money remuneration is offered, but duplicate specimens may be retained by the authors. Unique specimens will be handed over to the South African Museum in Cape Town.

A BIOLOGICAL Survey of Alabama has been organised and put into operation. The Survey will be carried on under the auspices of the Alabama Polytechnic Institute by the specialists engaged at that institution in the various lines of biological investigation. Its object is to study in field and laboratory all plants and animals occurring in the State, and the various conditions affecting them. The work will be done systematically and thoroughly, and all results published. In a region so interesting and little worked as this portion of the Southern United States, careful and extended research will be sure to yield results of the greatest value, and we hope that Alabama biologists will receive the support they deserve. Large quantities of material in all groups of plants, and of animals (especially insects) will be collected and properly prepared. In connection with the Survey there has been founded an Exchange Bureau, from which will be distributed all duplicate material. Any desiring to correspond relative to specimens, literature, or the work of the Survey, should address: Alabama Biological Survey, Auburn, Alabama.

EVEN the hardened scientific man, in all his pride of ignorance of the Greek language, history, and culture, may feel some thrill of interest in hearing that Professor Dörpfeld, in his excavations to the N.W. of the Areopagus, has discovered one of the potsherds by which Themistocles, the great and mean, was ostracised in 471 B.C. On it are impressed the words "Themistocles Phrearrios," the latter having reference to the deme in which the statesman was born.

IN connection with the Essex County School of Horticulture, a botanic garden and demonstration ground is to be established on three acres of land acquired at Rainsford End, Chelmsford.

The *Scottish Geographical Magazine* states that an experimental station has been formed in Zanzibar for the study of the agricultural resources of East Africa, and the suitability of various crops for cultivation in that region.

G. Zenker, formerly Director of the Jaunde Station in the Cameroons, has set up in Bipinde a research station for the study of the fauna and flora of this district.

The Government of Dutch India has appropriated $6,000 for the erection of a research laboratory at the Buitenzorg Garden.

A SCIENTIFIC expedition to the Far East has been organised by the German Government, and will probably start on January 27 from Bremen on board the North German Lloyd s.s. "Sachsen."

A meteorological station, in connection with the German Antarctic expedition, is shortly to be established under the direction of Dr. Rudolph Mewes, in Victoria Land.

DR. O. NORDENSKIÖLD has sailed in the Chilian vessel "Magellanes" for the South Shetland Islands, where both coal and gold are said to occur.

Nature announces that J. Graham Kerr and J. S. Budgett, both of Cambridge, who started last August for Paraguay, have been successful in their quest for specimens of *Lepidosiren paradoxa*, and will shortly return with abundant material to work out.

Mr. C. G. Pringle has returned from his annual trip into the more unknown regions of Mexico with about 20,000 zoological and botanical specimens.

THE Vienna Academy has sent Drs. H. Müller, Ghon, Albrecht, and Pöch to investigate the bubonic plague in India. Their expenses will be paid out of the Treitl Fund.

Reuter states that Dr. Koch has discovered that cattle can within a fortnight be rendered immune to rinderpest by a mixture of serum and virulent rinderpest blood.

CORRESPONDENCE.

Mr. Jackson's Map of Franz Josef Land.

A friend has just drawn my attention to an article which appeared in the January issue of Natural Science, and as this article forms an attack—an anonymous attack—on Mr. Jackson's work in Franz Josef Land, and criticises my report of that work in the *Geographical Journal* of December last, I must ask you to give my reply the same publicity you afforded the series of inaccurate statements which I shall now proceed to refute. I need scarcely add that I shall not comment on the animus or the singular taste of this anonymous attack on a man who is still at work in the Arctic Regions, and unable to reply for himself.

(1.) But it may clear the ground, in the first place, if I deal with the way in which you have sought to insinuate that I was belittling the magnificent work of Dr. Nansen. Nothing was or could be farther from my intention, and the following extracts from my paper will clearly show this.

Writing of Nansen's safe return on the "Windward," I said:—"The delight and the wonder of it were not confined to what we call 'geographical circles,' but commanded the attention of that world which honours the courage and applauds the prowess of the polar explorer—a world which is as wide as our globe, and only limited by the distribution of the human race."

Anxious also to show how my absent friend appreciated the great explorer's work, I quoted from a private letter of Mr. Jackson's as follows:—"I told him [Nansen] how intensely pleased I was to be the first person to congratulate him on his magnificent success."

And again:—"I have done my utmost to make him comfortable, and to give him a good time after his rough experiences. He has made a most extraordinary journey, which, for daring, is, in my opinion, absolutely unequalled in the annals of discovery, either in the Arctic or other Regions."

Further, you begin your article with "Well! we *have* made a mistake. . . . We thought he (Nansen) had achieved a magnificent success. But a paper by Mr. A. M. Brice in the December number of the Geographical Society's *Journal* shows up our error." You then quote a portion of a passage which I will, with your permission, quote more fully. I was pointing out how much more has to be done in the North Polar regions, and my words were these:—

"The journey of Nansen—memorable as it must remain—still leaves immense areas absolutely unknown. The drift of the *Fram*—full of marvel though it be—has only extended our knowledge a few degrees in one direction. [Her marvellous drift was roughly on a W.N.W. course from the New Siberian Islands to the longitude of Spitzbergen.] But there are more degrees and many directions. The unknown geography of the North Polar world still invites attack, and still holds out the promise of a crowning reward."

To put it in a single sentence, I may say that as long as the larger half of the North Polar basin is still undiscovered, the scientific geographer cannot and will not be satisfied. And to show that I was not alone of this opinion, I will quote one remark made on that occasion by the President of the Royal Geographical Society:—"As Mr. Montefiore Brice has said, there is much more to be done."

(2.) In the light of my quotations in the paper from Mr. Jackson's letters, your statement—"We turn eagerly to see what were the achievements wrought in Franz Josef Land beside which those of Nansen pale"—is a gross inversion of the plain words Jackson wrote on Nansen's work:—"absolutely unequalled in the annals of discovery."

(3.) Referring to the discovery of Cape Mary Harmsworth and my (not Jackson's) identification of this with Gilies Land, you write:—"Interested in this great

discovery, we turn to the map only to recognise in the 'newly-found cape' our old friend Cape Lofley." This is a statement, I submit, absolutely unwarranted by the facts. The map made by Jackson and Armitage clearly accounts for the cape which Mr. Benjamin Leigh Smith saw in 1880 beyond C. Ludlow, and called C. Lofley; and it also accounts for the land which Mr. Smith saw in 1881 and laid down as connecting them. Jackson and his colleagues travelled close under the coast the whole way; they ascended, for the first time, nearly all the capes and elevations on the route, and made observations and took bearings from them. And then, on rounding C. Lofley—which was unmistakeable—they opened up, for the first time, a third cape, and this cape is C. Mary Harmsworth. You say that Jackson has removed Mr. Smith's C. Lofley to "an insignificant point," and that C. Mary Harmsworth is Mr. Smith's C. Lofley. My reply to this is that there is no insignificant point on that line of coast which could possibly be mistaken for a cape, and that C. Lofley and the intermediate stages were all and successively identified before reaching C. Mary Harmsworth.

(4.) Then you give what you call "another instance of the Jackson-Harmsworthian method of treating the names of their predecessors"; and state that Jackson has re-named Payer's Markham Sound as the British Channel—adding that Mr. B. L. Smith "accepted the name and mapped the south-western end of the sound." My reply is that Mr. Smith acted rightly in accepting the name, because we now know that he never entered Markham Sound or mapped its south-western end. Jackson, however, has discovered in the British Channel a wide sound—another and more important "Austria Sound"—to the west of Markham Sound, and to the west, even, of Payer's Zichy Land. A knowledge of Payer's map might have saved Jackson's nameless critic from this error. For in Payer's map Markham Sound washes the north of McClintock Island, and this is precisely what Markham Sound still does in Jackson's map. Payer's south-west prolongation of the Sound was guess-work: Jackson's actual survey cuts that prolongation short with a chain of islands. And so little is the British Channel identical with Markham Sound that it is nowhere near McClintock Island, but far away to the west of the Sound, and even west of Payer's Zichy Land.

(5.) Again, you write: "Mr. Jackson's detailed map of the southern coast of Alexandra Land will doubtless be of great value when this part of the world is divided into building lots." If this is the opinion of NATURAL SCIENCE on the importance of accurate and detailed mapping, I may pass on, merely quoting the opinion expressed by Sir Clements Markham (whose name, by the way, NATURAL SCIENCE misspells) on the same point:—"Such a survey will be a most useful and valuable contribution to geography—its value, indeed, can hardly be exaggerated."

(6.) In citing one of my quotations from Mr. Jackson's letter, you print it thus: "At his [Nansen's] winter hut, he believed himself near C. Lofley, and that the land to the westward, which we had discovered (!), was Spitzbergen." What, may I ask, is the object of this interpolated (!)? The land Nansen saw to the westward was the land marked 21, Island 10, and Island 11 on Jackson's map. Your note of exclamation would seem to express doubt or affect surprise at Jackson having discovered this land. It is, however, perfectly certain that he was the first to discover (in 1895) that and all the adjacent land, including the island on which Nansen subsequently wintered. This is beyond all doubt, and your insertion of the note of exclamation is merely offensive.

(7.) You appear unable to understand the generous feelings expressed by Jackson for Nansen, and his request that Nansen should name the island and locality where he wintered you describe as his "gracious permission to re-name an already named island"—Karl Alexander Land. Here, again, you are wrong, and, as you appear to base your opinion on the map in the *Geographical Journal*, December, 1896, I may inform you that I requested the Society's cartographer to insert Payer's map in lighter colours, in its proper place to the east of Jackson's field of work, and without attempting to correct, at the points of contact, the obvious discrepancies. Inference from the areas in contact is consequently useless, unless guided by knowledge; and Karl Alexander Land is *not* identical with the island on which Nansen wintered. Jackson knew this and Nansen knew this, as the former proved by asking Nansen to

name it, and the latter proved by calling it after my absent friend, the leader of the one Arctic expedition still at work in the field.

Finally, I must record my protest against these ignoble imputations, these gross inaccuracies, this arm-chair view of Polar work, and this "stabbing in the dark" of a man who is now wintering for the third time in Franz Josef Land, in the cause of geographical science.

ARTHUR MONTEFIORE BRICE.

157 Strand, W.C.,
February 12, 1897.

P.S.—In studying the new map of Franz Josef Land (*Geographical Journal*, December, 1896) it should be remembered that the discoveries of Payer and Smith occasionally overlap with those of Jackson. The latter is only responsible for areas enclosed in heavy lines and names printed in heavy type. A. M. B.

[With Mr. Brice's epithets we deal elsewhere. The geographical questions raised by him are: (3) the relations of C. Mary Harmsworth and C. Lofley; (4) the use of the name Markham Sound; (6) the discoverer of land seen by Nansen to the westward of his winter hut; (7) the name of the island on which Nansen wintered. The late date at which we receive this letter prevents us reproducing any maps, reference to which would easily show geographers the correctness of our previous remarks. These we will attempt to elucidate for Mr. Brice's benefit, as follows:—

(3.) C. Mary Harmsworth is identical with C. Lofley. Leigh Smith saw the coast from a position whence he must have seen the cape called C. Mary Harmsworth by Jackson, and whence (as any geographer but an armchair one will understand) he could not have distinguished the point so obvious to Jackson's party working along the coast. Anyone who will plot Leigh Smith's map on to Jackson's so that C. Ludlow corresponds, will find that C. Lofley of Smith coincides with C. Mary Harmsworth of Jackson.

(4.) As for Markham Sound, we wrote with Payer's map lying before us. This shows that he gave the name to a sound running along the S.E. coast of Zichy Land, which he saw and did *not* guess at. Those who have not Payer's map may refer to the map distributed in illustration of Mr. Brice's own paper at the Geographical Society on November 11, 1895, in which map Markham Sound is represented as separating Zichy Ld. from the islands there grouped under the name Hooker Id. That is the exact position now usurped by Jackson's "British Channel." Further, Payer's Markham Sound runs N.E. and S.W., but Jackson's runs N.W. and S.E.

(6.) We are most willing to admit Jackson's claim to be the discoverer of "the land marked 21, Island 10, and Island 11." But these three small patches are not quite the same as "the land to the westward" of Nansen's winter hut, for this obviously included Zichy Land.

(7.) Nansen wintered on Payer's Karl Alexander Land. Comparison of the maps by Payer, Jackson, and Nansen shows that Hooker Id. and the three islands to the north, were regarded by Payer as the western end of his McClintock Id. North of this is the island with the high ground or mountain which Payer named after Richthofen. Then, crossing Payer's Todesco Fjord, we come to another island, to the N.W. point of which Jackson applied the name of McClintock. It is the next land to the north on which Nansen wintered, and this is the S.W. end of Payer's Karl Alexander Land. This, however, appears from Nansen's map to be three islands, of which the northernmost is a westerly continuation of Payer's Andrée Id., and should bear that name.

Mr. Brice's laborious explanation of the map illustrating his last paper was quite unnecessary; he has forgotten that it was given quite plainly on the map itself. The "obvious discrepancies" between the maps of Payer and Jackson are mostly due to the fact that parts of Payer's map were laid down from observations made at a distance, and are therefore not so correct as those on his actual line of march. Mr. Brice should beware of laying stress on this, or people will infer, most unjustly of course, that he is trying to minimise the results of the Austrian explorer as well as those of the Norwegian.—EDITOR NATURAL SCIENCE.]